The Journal *of*
Parapsychology

Volume 11 SEPTEMBER, 1947 Number 3

EDITORIAL

PIERRE JANET'S CONTRIBUTION TO PARAPSYCHOLOGY

We recently addressed an inquiry to the eminent French psychiatrist, Dr. Pierre Janet, inviting him to contribute an article on his observations and reflections relating to parapsychology. We received a reply from a member of his family containing the information that Dr. Janet had died in February of this year.

Dr. Janet was one of the founders of psychopathology and psychiatry, and were our pages devoted to those subjects, it would be fitting to offer a full appreciation of the work of one who was formerly a great leader in the psychological sciences. Our remarks, however, must naturally be confined to the activities of Dr. Janet in the field of parapsychology. Janet was not regarded as a parapsychologist and he himself would have been the last to have assumed such a characterization. He was a corresponding member of the Society for Psychical Research of London, but he was not publicly associated with its work; his contribution to parapsychology was not extensive, and it was not especially conclusive. His observations were not even published in full by Janet himself.

What makes his work in parapsychology of special interest is the fact that he was deterred from a more exhaustive pursuit of phenomena that were so interesting and challenging as those credited

to his observation. That a pioneer like Janet can be stopped while on a trail offering such promise for psychological science constitutes something of a historical exhibit, a demonstration of the social forces under which the explorer in parapsychology has had to work. Such a picture definitely belongs in any true and complete account of the research of this field.

Dr. Janet's brief two-year excursion into parapsychology occurred when he was a young physician at the age of twenty-six. One day (Janet wrote in an article in the *Revue Philosophique* of February, 1886) when a colleague, Dr. Gibert, was holding the hand of a patient, Léonie B., to hypnotize her, his attention wandered and he found that the subject did not go into trance. This experience suggested that it was what the hypnotist *thought* rather than what he *said* that was effective in this case. Experiments were made to test this hypothesis, and, as a result, the conclusion was indicated that Léonie B. could be hypnotized by the transfer of thought from the hypnotist without any apparent intermediating sign.

Dr. Gibert, with the collaboration of Dr. Janet, undertook then to put Léonie into the hypnotic state at a distance. A series of trials were made with a distance of a kilometer between the subject and the hypnotist. In some of these Gibert was the operator and in others, Janet. Both hypnotists were quite successful in bringing about the hypnotic trance approximately at the hour agreed upon, the selected time ranging from 11:00 A.M. to 11:35 P.M. It was necessary to depend upon the members of the household in which Léonie B. was staying for a record of the time when the trance began. No one present in the house knew of the experimental plan.

Out of twenty-five trials made in 1885, as reported by F. W. H. Myers in Volume 4 of the *Proceedings of the S.P.R.*, there were eighteen complete successes, with four partial or doubtful successes. (In his autobiography in 1930, Janet states that "16 times out of 20 somnambulism has exactly coincided with a mental suggestion made at a distance of one kilometer.") While it is not possible to obtain an accurate estimate of the probability of such a result, the proportion of successes seems very impressive, and it was so regarded at the time by Myers and by his brother, Dr. A. T. Myers,

who witnessed some of the trials and obtained an account of the others.

What Dr. Janet's attitude toward these observations was and what interpretation he gave them at the time remain unknown. His colleague and friend, the eminent physiologist, Charles Richet, referred to him, when writing about his work in parapsychology, as "a careful and skeptical observer." The fact that Janet allowed Richet to publish the major part of his observations instead of reporting them himself might suggest indifference. But Janet did report the results of his first series and then went on to conduct a still longer series consisting of 35 trials. These facts alone are sufficient to rule out indifference as an explanation. The second series did not produce so many hits; there were only nine cases regarded as successful, with six doubtful ones. Even without statistical evaluation we may be reasonably sure that Janet did not regard these results as due to chance. Yet here is where he ended his active contact with psi phenomena. From 1887 on, we find Richet working with Léonie B., with no later work attributed to Janet.

There were, however, other experimental and incidental observations made by him on Léonie in the period 1885-86. Richet states in his *Thirty Years of Psychical Research* that Janet tested her for the telepathic transfer of taste, using common substances such as salt, sugar, pepper, etc. The report is lacking in details, but it states that Léonie was able to identify correctly the substance which the hypnotist put in his mouth. We do not know the precautions taken to exclude sensory sources of knowledge. From the same volume, we learn of the reaction of Léonie when Dr. Janet's brother, who was also engaged in the hypnotic experiments with Léonie, scorched his arm in an adjoining room while Dr. Janet himself was engaged in putting Léonie into a hypnotic state. Janet knew nothing of the accident until Léonie gave signs of pain and showed the spot on her arm where the pain was localized. It coincided with the spot injured on his brother's arm.

Richet goes on to report that one day Janet sent Léonie "traveling" clairvoyantly while in hypnotic trance. She was asked to go to Paris and visit Dr. Richet's laboratory. The place of her own location at the time is not stated, though it was probably Le

Havre. Suddenly she said, "It is burning," and seemed much excited. Dr. Janet, who was ignorant of any significance to be attached to her words, tried to calm her, but again she said, "I assure you it is really burning." On that same day quite early in the morning Dr. Richet's laboratory had in fact caught fire and was burned out.

Forty-four years later, after referring to the distance tests with Léonie which he conducted, Janet wrote: "These experiments, which the representatives of supernormal psychology have published and popularized in my opinion too soon, have since that time been cited and used in all works on the unknown faculties of the human mind. In viewing these citations and this abuse of my former observations, I have always had a feeling of astonishment and regret. Strange that these authors who reproduce with such confidence these experiments of 1882 [actually 1885-86] have never had the idea of writing to the experimenter who was still living and asking what he thought of them! I should have answered that already at that time, and even more so now, I doubted the interpretation of the facts and was disposed to criticize them myself, regarding them as a simple departure from more profound studies."

The late Professor William McDougall once stated that on meeting Dr. Janet he asked him what he himself thought of the results of his experiments on hypnotization at a distance. Janet's answer was a shrug of the shoulders and the remark, "What is one to say? These are very difficult matters."

As judged by present standards of evaluation in parapsychology, the character of these observations is more provocative than conclusive. They are, however, sufficiently striking to cause wonder that they were not more vigorously followed up by systematic investigation. We cannot suppose that their significance, so fully appreciated by men like Richet, was completely overlooked by Janet himself.

We must, of course, consider Dr. Janet's professional position at the time. He was a young physician, already pioneering in a field that in itself was unorthodox in 1886. His special interest was in psychopathological rather than parapsychological phenomena. We may recognize, too, and without uncomplimentary reflection,

that he was evidently somewhat conservative in nature. While his contributions in his chosen field are, indeed, of great value and show originality—particularly his theory of the dissociative character of mental disorder, his views of psychical energy, and his methods of psychotherapy—still his reaction to the more revolutionary psychological developments of his day was one of reserve. Moreover, it would have been a bold step for any young medical man of the eighties to have declared himself at work on psi phenomena of any type.

Even though we may regret that Janet did not use his influence to encourage and recognize the early parapsychological explorers of his day who were courageously conducting and reporting their studies, we must recognize that such reactions are largely determined by the intellectual environment to which the individual belongs. The whole culture itself would have to be indicted if any charge is made against the individual. Dr. Janet no doubt knew only too well how his medical colleagues would have regarded any publication of his that told of his unexplainable observations made on Léonie. It required some courage for a physician in the eighties even to dabble in such unorthodox areas as hypnosis and the phenomena of mental disease. If one is to succeed in an unorthodox field he had better be orthodox in his other connections.

We cannot rightly judge what young Dr. Janet should or should not have done in 1886 when he gave over his parapsychological work with Léonie B. Rather, we may, and in propriety should, acknowledge that his short adventure into parapsychology left an effect that was quite important to that incipient science. There can be no doubt that Janet, in his collaboration with Gibert, helped substantially at a very strategic time to bring attention to the strange and easily overlooked phenomena of telepathy associated with hypnotism. Even without a published report from Janet and Gibert, other physicians were led to look for similar effects and, in the years that followed, to report confirmatory findings. We may be sure that Janet's observations lost nothing of their value in spite of the reluctance of the explorer himself to release his findings to the scientific world.

J. B. R.

SIMULTANEOUS HIGH AND LOW AIM IN PK TESTS

By Betty M. Humphrey

ABSTRACT: The experiment described here by Dr. Humphrey might be called a "double-aim" test. It was a PK experiment in which two sets of dice were mixed together and thrown from the same cup at the same time while the subject had a different target in mind for each set. For example, at the same time that she was trying to get as many ones as possible on six red dice, she was trying to get as few as possible on six white dice. An equal number of throws were made for the high aim and for the low aim with each kind of dice. In the total of 64,800 die throws a greater number of hits was obtained on the high aim than would be expected from chance, and a lower number of hits on the low aim. The difference between the two results is significant. Dr. Humphrey makes a plausible case for the view that the subject really is effectively succeeding in two "directions" at once. This work adds a further item to our knowledge of what PK can do.—Ed.

Introduction

From studies on the types of target combinations with which PK can successfully operate, valuable hints may be gained as to certain characteristics of PK and as to its method of operating. It has been found that PK can successfully operate in tests in which combination targets such as sevens and high dice are used (**2, 3, 7**). The present experiment was carried out as a further variation in the study of target combinations in PK tests, the purpose of the investigation being to see if a combination of high aim and low aim can be successfully used in PK tests. Can the mind influence two kinds of dice *at the same time* so that positive results are obtained on one and negative results on the other? In an attempt to answer this question, I (serving as my own subject) conducted this first exploratory experiment in which I threw two different kinds of dice from a cup at the same time and attempted to will them to fall so that an excess of hits for the desired face would be obtained on one set of dice and a deficiency of hits for the same face would be obtained on the other.

The Experiment

Two concurrent series of PK tests were planned and the number of trials in each was decided upon before I started the experiment in February, 1944. In both series the target was always the one-face. In Series A, six red dice were thrown at a time for ***high*** scores on the one-face and six white dice were thrown for ***low*** scores on the one-face. All 12 dice were thrown at the same time from the same dice cup. In Series B, the six red dice were thrown for ***low*** scores on the one-face, while the six white dice were thrown for ***high*** scores for the one-face.

Each series consisted of a total of 1,350 runs (of 24 die throws each), 675 runs for high aim and 675 runs for low aim. Thus, in the experiment as a whole, each kind of dice was used in 675 runs for high aim and in 675 runs for low aim on the one-face. The experiment was planned in this way so that any results obtained could not be interpreted in terms of biased dice. By reversing the target, any positive scoring which was due to biased dice in one section would be compensated for in the second section when the same dice were used for the opposite aim.

The record sheets for both series were of the type illustrated in Figure 1. The record page consisted of six "sets," each set having six columns with six entries in each column. In both series the entries for the high-aim tests were always recorded in the sets on the left side of the page, and those for the low-aim tests on the right side of the page. Since 12 dice were thrown at a time (six for high aim and six for low aim), there was a total of 54 runs made on each page. In order to obtain the goal of 1,350 runs for each series, it was necessary to complete in each series 25 record pages of the type illustrated.

At first I had planned to alternate pages, doing first a page in Series A and then a page in Series B, and so on. During the first few pages, however, I found it difficult to change my mental attitude; after I had been trying, for example, to get ones to appear on the red dice and no ones on the white dice, it seemed very hard to change suddenly and try to accomplish just the opposite. Thus I soon adopted the practice of doing one or two pages in one series and then waiting several hours before taking up the other series.

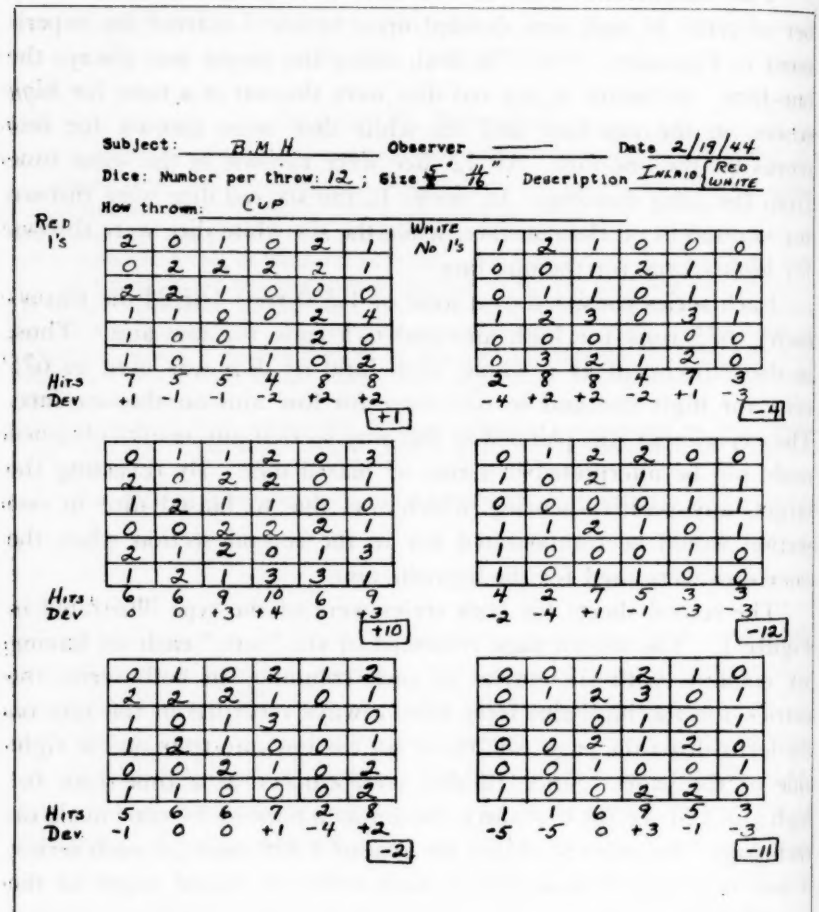
Subject: B.M.H Observer —— Date 2/19/44
Dice: Number per throw: 12 ; Size 5/8" 11/16" Description Inlaid {Red White
How thrown: Cup
Red 1's
White No 1's
Hits 7 5 5 4 8 8
Dev +1 -1 -1 -2 +2 +2 +1
Hits 2 8 8 4 7 3
Dev -4 +2 +2 -2 +1 -3 -4
Hits 6 6 9 10 6 9
Dev 0 0 +3 +4 0 +3 +10
Hits 4 2 7 5 3 3
Dev -2 -4 +1 -1 -3 -3 -12
Hits 5 6 6 7 2 8
Dev -1 0 0 +1 -4 +2 -2
Hits 1 1 6 9 5 3
Dev -5 -5 0 +3 -1 -3 -11
Total High Aim
Runs = 27
Dev. = +9
Total Low Aim
Runs = 27
Dev. = -27

FIG. 1. Copy of a record sheet from Series A showing high-aim entries on the left and low-aim entries on the right.

Throughout both series I worked alone as subject with no observer present. I would decide in advance which series I was going to work on and then I would try to fix firmly in mind the target for each color of dice. For example, if Series A was to be run, I would repeat to myself that ones were to come up on the red dice and no ones were to appear on the white dice. All through the throwing I would try to keep this in mind by saying to myself such phrases as: "Ones on the red dice, no ones on the white."

The six red and six white dice were shaken together in a standard one-quart size dice cup which had a corrugated rubber lining. Then the dice were thrown out upon a padded dice table so that they bounced and rolled freely. When they had all come to rest, I counted the hits for high aim first and recorded the total number in the first column of the left set at the top of the page. Then the number of ones that came up on the "low-aim" dice were counted and the total recorded in the first column of the right set at the top of the page. In order to minimize the possibility of self-delusion, I carefully picked up the dice having the one-face uppermost and recounted them as a check on my original record. The dice were then returned to the cup, shaken, and thrown again. This time the entries were made in the second space in the first column of the left and right sets, respectively.

The thirty-sixth throw marked the end of two sets, one for each direction of aim. At this point the hits were totaled and the deviation from mean chance expectation was determined. An average of one hit per throw is the expected chance score for each color of dice. My aim, of course, was to obtain on each trial more than one hit on the high-aim dice and no hits on the low-aim dice.

At the end of each page, the total deviation for each kind of dice was determined, as is shown in Figure 1.

When the experiment was originally planned, I had intended to use 12 inlaid dice—six red transparent ones of the kind known commercially as "perfect" dice, and six white ones with tri-colored inlaid spots (the one- and six-faces had black spots, the two- and five-faces had red spots, and the three- and four-faces had blue spots). Actually, however, when I was ready to begin the experiment, the six "tricolor" white dice were being used in another experi-

ment and were not available for my tests. Therefore six white dice, 11/16 of an inch on the edge, were substituted. The spots on these were formed by holes marked with black paint. For almost a fourth of the experiment, these dice were used with the red transparent dice with white spots, which were mentioned above. It soon became apparent, however, that I was forming a very strong preference for the red dice. This seemed natural enough, for they were bright and attractive in comparison with the white dice with their dull finish and black spots. I realized that I found it easy to get low scores on the white dice when desired, but almost impossible to obtain high scores on them when high aim was called for. This effect might actually have been due, partly or wholly, to a bias in the dice caused by the excavated spots, a factor which would tend to disfavor the appearance of ones. At any rate, whatever the cause of my difficulty, I decided that the dice should be changed in order that both series might stand a more equal chance for success. The white inlaid dice having become available, I substituted them for the white dice with excavated spots. This change in white dice occurred after the fifth page in Series B and after the seventh page in Series A. All subsequent pages were made with these same dice, all inlaid. The table below shows which dice were utilized in each part of the experiment.

	Series A		Series B	
Pages	*High Aim*	*Low Aim*	*Low Aim*	*High Aim*
1-5	Red, Inlaid	White, Excavated	Red, Inlaid	White, Excavated
6-7	Red, Inlaid	White, Excavated	Red, Inlaid	White, Inlaid
8-25	Red, Inlaid	White, Inlaid	Red, Inlaid	White, Inlaid

This change in dice introduces a problem into the evaluation of the results. Since seven pages in Series A were made with the white dice having excavated spots and only five pages in Series B were made with these dice, it is possible that any bias in the dice might lead to spuriously high scores in favor of the specified targets; that is, if, as we suspect, the white dice with excavated spots are biased to favor low aim for the one-face, spuriously high results would be produced because more pages were made for low aim than were made for high aim with these dice. In the original plan of the experiment, an equal number of trials for high and for low aim were to be made with each type of dice in order to rule out the

effect of any bias in either type of dice. In keeping with this plan, then, it was decided to omit two pages in each series in such a way that there would be an equal number of trials for high and low aim for each type of dice. The omission of pages 6 and 7 from each series left five pages in each series which were made with the excavated dice and 18 with the inlaid white dice. (The red transparent dice were used, of course, in all pages of both series.) This omission is of importance mainly in the presentation of total results; it will be seen later that it was not necessary to do this in the evaluation of position effects.

Results

The total results for both series are given in Table I. These figures include only the 23 pages in each series in which the same dice were used an equal number of times for all conditions. From the table it may be seen that the total deviation on the high-aim sections of both series pooled is positive, while that for the low-aim sections is negative. The CR of the difference between the high-aim and the low-aim sections is 2.46, which is significant.[1]

Table 1

General Summary of Results

Series	High Aim				Low Aim				CR_d
	Dice	Runs	Dev.	CR	Dice	Runs	Dev.	CR	
A.....	Red	621	+68	1.50	White	621	−123	2.71	
B.....	White	621	−23	.51	Red	621	− 56	1.23	
Total...		1242	+45	.70		1242	−179	2.78	2.46

These figures indicate that it was possible to obtain significantly different total results in a combination high-aim and low-aim test. They show that it was possible to obtain high and low results in tests carried out simultaneously. They do not, in themselves, however, answer the initial question which led to this experiment;

[1] If the data from all 25 pages in each series had been included, this CR of the difference would be 2.66. As was explained above, the deletion of pages 6 and 7 from each series was done in the interest of equalizing the dice.

namely, can the mind do two different things at once in PK tests? The results just cited show us that the two things can be accomplished in the experiment as a whole, but they do not show whether both high aim and low aim were achieved on the same throw. It might be that high aim was accomplished on one trial and low aim on another. My own feeling, as I went through the experiment, was that I succeeded in keeping both aims in mind only toward the last of each set, but I recognize that this might be an illusory belief deriving from success in those runs. The stopping to compute the deviation after each set seemed to dispel the mental attitude and it had to be worked up again within the course of the next set.

In an analysis to determine whether the desired results of both high aim and low aim were obtained in the same trial, I counted the number of instances in which a positive deviation was obtained on the high-aim dice and a negative deviation was obtained on the low-aim dice on the same throw. For example, in Series A, a count was made of the throws in which a score of from 2 to 6 was obtained on the red dice and a score of zero was obtained on the white dice (one hit per throw is expected by chance for each kind of dice). Then in order to get a basis for comparison, a count was made of the number of throws in which just the opposite occurred—in which a low score was obtained on the high-aim dice and a high score was obtained on the low-aim dice. If chance alone was operating, the number of cases in each analysis would be approximately the same. On the other hand, if PK were accomplishing the result aimed at, there would be more instances in which the desired targets were obtained than those in which the opposite was found.

The total results for the two categories (successes versus failures) are shown in Table 2. (Only the 23 pages in each series in which dice bias can be ruled out are included in this analysis.) In all, there were 463 cases in which high scores (above chance) were obtained on high-aim dice and low scores (below chance) on low-aim dice at the same time. In contrast with these there were 399 cases in which low scores were obtained on high-aim dice, and high scores on low-aim dice. In the total of 862 trials which could be used in this analysis, we would expect by chance approximately 431 cases in each group (successes and failures). Thus we see that 32

more successes were obtained than expected and 32 fewer failures. On the basis of the total of 862 trials with a probability of one-half, we may compute the CR for the successes obtained. The CR is 2.18, which has a probability of .015. Thus the successes are not significantly different from those expected by chance, but they exceed chance expectation to a degree which *suggests* that PK was effecting the desired result.

Table 2

NUMBER OF SIMULTANEOUS SUCCESSES AND FAILURES IN THE THIRDS OF THE SET, BOTH SERIES POOLED

	Columns 1, 2	Columns 3, 4	Columns 5, 6	Total
Successes	151	146	166	463
Failures	145	132	122	399
Expectation	148	139	144	431
Dev. of Successes	+ 3	+ 7	+ 22	+ 32

Recalling my feeling of greater success toward the end of each set, I carried out the same analysis separately for each third of the set; that is, the number of successes and the number of failures (as defined above) were counted in the first two columns of each set, in the second two columns, and in the third two columns. The predominance of successes over failures occurred almost entirely in the last two columns of each set. (See Table 2.) The deviations of successes over the number expected by chance for the thirds of the set are, respectively, +3, +7, +22. The CR for the successes obtained in the last third of the combined set is 2.59, which is significant. Thus we see that the primary aim of the experiment was actually accomplished only in the last part of each set.

When we look at the results in terms of the sets on the page, we see that the successes are fairly evenly distributed, although there is evidence of a slight decline. (See Table 3.) Within each set, however, regardless of its position on the page, there is the general tendency for an increase in success toward the last portion of each set.

Table 3

NUMBER OF SIMULTANEOUS SUCCESSES AND FAILURES IN THE SETS ON THE PAGE, BOTH SERIES POOLED

	Set 1			Set 2			Set 3		
	Cols. 1, 2	Cols. 3, 4	Cols. 5, 6	Cols. 1, 2	Cols. 3, 4	Cols. 5, 6	Cols. 1, 2	Cols. 3, 4	Cols. 5, 6
Successes	56	49	55	47	56	52	48	41	59
Failures	46	42	38	51	45	42	48	45	42
Expectation	51	45.5	46.5	49	50.5	47	48	43	50.5
Dev. of Successes	+ 5	+ 3.5	+ 8.5	− 2	+ 5.5	+ 5	0	− 2	+ 8.5
Dev. per Set		+17			+8.5			+6.5	

POSITION EFFECTS

Quarter Distributions of the Set

At the time this experiment was begun, parapsychologists were particularly interested in the quarter distribution studies of the PK data (**4, 5**). It had been generally found that the first quarter of the set or page showed a significantly greater number of hits than did the last quarter of the scoring unit. For the present series, the QD of the set was obtained for both the high-aim and low-aim data. Since the evaluation of the QD depends upon internal differences within the set (mainly differences between the first and fourth quarters), it is not necessary to omit the two pages for each series in which different dice were used. It will be recalled that in pages 6 and 7 of Series A, the white dice with excavated spots were used, while in pages 6 and 7 of Series B the white inlaid dice were used. These pages were deleted only in order that the comparison of high-aim and low-aim sections would be free from the influence of any bias in the dice. They can now be included in the position effect studies since the comparison here depends upon differences *within* the set for each section of the data separately, and of course the same dice were always used throughout each set.

The results of the QD analysis are given in the squares below where the deviation for each quarter of the set may be seen together with the total number of runs involved in each.

QD's of the Set

High Aim

1350 Runs: + 54

337.5 -29	337.5 +8
337.5 +1	337.5 +74

Low Aim

1350 Runs: —198

337.5 -22	337.5 -75
337.5 -84	337.5 -17

The QD for the high-aim section is exactly opposite that generally found in PK data. The first quarter is below chance expectation, while the largest positive deviation occurs in the fourth quarter. The CR of the difference is 2.17.

In interpreting the QD for the low-aim data we must remember that a negative deviation was desired. Thus, the greater number of successes were obtained primarily in the second and third quarters. This is the first instance in the literature in which low-aim tests of PK have been done; thus it is not possible to compare these position effects with others of the low-aim variety. On the basis of the other PK studies, however, we might have expected the greatest negative deviation (the desired result) to appear in the first quarter. It is probable that the result shown in these series is mainly due to the type of test carried out here and that it will not be found generally in low-aim PK tests. This view is supported by the fact that the QD for the high-aim data did not follow the trend found in other high-aim PK series (all of the published work so far has been of the "high-aim" type).

The appearance of these nontypical QD's is not unexpected in view of the special conditions obtaining here. The difficulty of keeping two things in mind at once would seem to be the factor most probably responsible for the change in the QD pattern.

The QD's are not, of course, completely independent of the

results presented just previously in which it was found that the complete successes (those in which both high aim and low aim were accomplished in the same trial) appeared to be localized in the last third of the set.

Horizontal and Vertical Distributions

In analyzing the set for the horizontal and vertical distributions, the entire batch of 25 pages in each series may again be used since the primary interest here is the study of position effects within each set and not a comparison of high-aim and low-aim sets.

In order to obtain the horizontal distribution in the set, the scores in the first column are added, then those in the second column, and so on, as shown in the diagram below. For the vertical distribution, the scores in the top row are added across, then those for the second row, and so on. The vertical distribution for the set is shown at the right, and the horizontal distribution at the bottom of the diagram, along with the corresponding deviations.

						Total Hits per Row	Deviation
2	0	1	0	2	1	6	0
0	2	2	2	2	1	9	+3
2	2	1	0	0	0	5	−1
1	1	0	0	2	4	8	+2
1	0	0	1	2	0	4	−2
1	0	1	1	0	2	5	−1
Total Hits per Column:		7	5	5	4	8	8
Deviation:		+1	−1	−1	−2	+2	+2

By combining the deviations for this set with the deviations for all the other sets, a composite horizontal and vertical distribution for the whole experiment is obtained. The results of the horizontal and vertical analyses for the entire experiment are shown graphically in Figure 2. The distributions for the high-aim sets are given on the left and those for the low-aim sets are given on the right. In interpreting the latter, it should be kept in mind that *negative* deviations were desired in this section of the experiment.

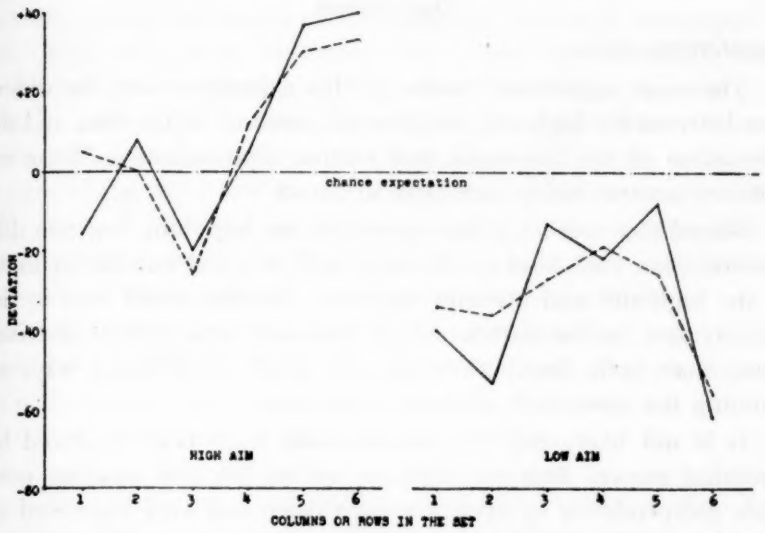

FIG. 2. Curves of the deviations for the horizontal distribution (solid line) and for the vertical distribution (broken line) for both the high- and low-aim tests.

Both distributions for the high-aim sets show a general U-curve effect, while those for the low-aim sets show an inverted U-curve. Even without the aid of statistics, it is clear to see that there is a striking correlation between horizontal and vertical distributions for both the high-aim and low-aim data. When the deviations for the horizontal distribution are correlated with those of the vertical distribution (by the Pearson product-moment formula), the resulting correlation coefficient is +.89 for the six pairs of deviations in the high-aim section of the experiment. A similar analysis for the six pairs in the low-aim section gives a correlation coefficient of +.84. Although these coefficients are quite high, the number of cases in each instance is small. The probabilities associated with the correlations for the high-aim and low-aim sections are slightly less than .02 and .03 respectively. A combination of these probabilities (**6,** Appendix 12) gives .005 as the probability associated with the relationship between the horizontal and vertical distributions in the entire experiment. This indicates that the two distributions are correlated to a degree expected by chance alone once in 200 times.

Discussion

Counterhypotheses

The more significant results of this experiment are the difference between the high-aim and low-aim sections of the data and the correlation of the horizontal and vertical distributions. These results are not reasonably ascribable to chance.

Biased dice could not have produced the high-aim, low-aim difference, since each kind of dice was used an equal number of times in the high-aim and low-aim sections. Neither could bias in the dice account for the correlation of horizontal and vertical distributions, since both distributions are the result of different ways of counting the same trials with the same dice.

It is not likely that the results could have been produced by statistical errors, since the high-aim versus low-aim analyses were made independently by myself at least twice and were confirmed by the other analyses, such as those for position effects. The computation of CR's and correlation coefficients was done independently by myself and another person.

An experiment of this kind, where the experimenter works alone, is open to the possibility that some will find it more logical to interpret the results as being the product of conscious or unconscious errors in counting and recording. Against such reasoning there is no defense except to point out that a discovery is never established on the basis of one experiment. The present experiment is merely the first exploratory one on the simultaneous-aim problem.

Comments

This experiment was carried out in the attempt to learn whether the mind can influence two kinds of dice at the same time so that different results are obtained on the two. From the present data we can at least answer that this can be accomplished to some extent. Although both high-aim and low-aim successes were obtained in the experiment as a whole, only in the last third of each set were both obtained in the same throws to a significant extent.

U-curves in horizontal and vertical distributions are familiar fixtures in the literature of parapsychology; but seldom, I believe,

have they shown the degree of similarity discovered in this experiment. Not only are the horizontal and vertical curves similar, but the horizontal curve for the high-aim data is similar to that for the low-aim, and the two vertical distributions are also alike. Comparison of high-aim curves with low-aim curves is made easier if the signs of the deviations for the low-aim curves are reversed. For both curves, the greatest amount of deviation in the desired direction occurred on the first and last positions in the curve, while the least amount of deviation occurred in the middle positions.

These curves are reminiscent of the curves obtained in the ESP work of Pegram (1). In her high-aim ESP card tests, she found that more correct calls were made toward the beginning and end of the deck; while in her low-aim ESP tests, more misses (the desired result) were obtained in the first and last groups of five trials in the deck. Her results are comparable to the horizontal distributions in the present PK series. In her PK work with dice, which consisted of parallel high-aim tests with high dice and low dice as the targets, the same subject showed the familiar U-curve of success within the column.

At the same time, we know that not all subjects who score significantly in ESP or PK tests give this particular internal distribution of hits.

The facts seem to require an explanation of position effects in terms of two major principles. On the one hand, they are evidence of the operation of fundamental laws governing the organization or structuring of mental activities, such as the Gestalt psychologists have emphasized. On the other hand, the differences found among subjects suggest that personality factors place the subject's personal stamp upon the manner in which the more basic, general principles show up in the test results.

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A REVIEW OF THE EVIDENCE FOR DOWSING

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The most conspicuous characteristic of psi, and the most troublesome, is its unreliability. As soon as the stage in research is reached when ESP and PK can be dependably demonstrated on demand, parapsychology will have surmounted its greatest obstacle. Rhine has pointed out that the cause of the difficulty is the fact that psi is unconscious (**6**). A subject in an ESP test has no awareness of whether his response is right or wrong. Finding a way of making psi conscious, then, should be one point of attack on the problem of the elusiveness of psi.

There may, however, be other ways around this problem. Perhaps some new technique of testing can be used which will enable subjects to score consistently well even though they have no conscious awareness of their success during the test.

In his latest book (**5**) Rhine has suggested: "If we are dealing with unconscious processes, why not use unconscious responses to record the key impressions and keep the whole process unconscious?" We know that a subject in an ordinary ESP experiment often becomes introspective and is inhibited in his performance by the conflict he feels in attempting to guess a card. Now if the response which he makes consists in unconscious muscular activity, we may expect the conscious inhibitions to be absent and his level of scoring possibly higher.

This method of attacking the problem of psi control is not merely a possible alternative to the approach of finding a way to make psi conscious. It may also be a means of hastening the discovery of the latter method through providing easily obtained, reliable evidence which may be studied for clues.

In considering the advisability of this line of approach we should consult the literature on automatisms to see if there is support for the view that ESP may be shown by means of unconscious responses.

There are several techniques which involve unconscious muscular action. Among these are automatic writing, table-tipping, and the use of the following instruments: the divining rod, the pendulum, the planchette, and the ouija board. These techniques are, of course, not new, and a full review of the literature of all of them would require a paper of tremendous length. I have therefore selected only one of these instruments for detailed discussion, and that is the divining rod, the use of which is called "dowsing." Most of what may be said with regard to the use of the divining rod applies as well to the pendulum, for the two automatisms are very similar in operation, and both have been used for the same purposes.

The divining rod has been so closely associated, in the minds of most people, with superstition, self-deception, and even trickery that it may seem a waste of time to discuss it seriously in a scientific journal. Yet, in spite of the admittedly doubtful character of dowsing practices, there is a rather challenging collection of cases regarding the dowser's claims.

The aim of this paper is not to reach a conclusion regarding the dowsing hypothesis. It is rather to examine the types of evidence and to survey selected cases of purported success in an attempt to appraise the suitability of the problem for further parapsychological research.

* * * * *

Dowsing is the name given to the practice of discovering, without geological or local knowledge, the exact location of any underground supply of water, ore, or other substances. This is done by means of an instrument, usually a forked twig, which is called a divining rod. The dowser grasps the rod by its two ends, holding it at an angle with the ground. The assumption is that when he comes over underground water, the twig, seemingly of itself, twists downward or upward, depending upon the dowser, thereby indicating the presence of water. Although the rod is usually a twig (preferably of hazel, willow, or peach) it may be a wire or a watch chain and often is a pendulum. Even such unlikely objects as a book, a knife and fork, smoking pipes, and a sausage are recorded as having been tried. Some dowsers entirely dispense with a rod, and locate water by holding their bare hands out in front of them

or simply by their sides, trusting to certain sensations to guide them.

Much has been written on the subject of dowsing. Most of the articles come from England, France, Germany, Belgium, and the United States. Undoubtedly the most penetrating and scholarly attack on the problem was made by Sir William Barrett, an English physicist. He, together with Theodore Besterman, published a book in 1926 called *The Divining Rod* (3) in which the authors summarized all the information which Barrett had collected in his twenty-five or more years of careful investigation. Nothing on dowsing has since been published which can compare with this work.

Barrett and Besterman reached the conclusion that the finding of underground water by means of a rod, without sensory cues, is a fact which can be explained only in terms of clairvoyance plus automatic response, and they supported these conclusions with well-documented cases.

Most of the history, evidence, and theory which are given in the present paper have been obtained either from Barrett's articles in the *Proceedings of the Society for Psychical Research* (1, 2) or from Barrett and Besterman's book. The other writings studied were, for the most part, either lacking in scope or simply puerile in comparison.

Recently articles on dowsing, written from a popular viewpoint, have appeared in the *Country Gentleman* (7) and the *Farm Quarterly* (8). In 1917 the United States Geological Survey put out a bulletin (4) designed to meet the many requests for information which they receive concerning dowsing. (It was reprinted in 1938.) The author, Arthur J. Ellis, gives a brief history of the subject, discusses various investigations and investigators, describes dowsing instruments, and reaches the conclusion that water divining is a superstition and a waste of time. Although this is the attitude taken by many writers, most of the literature on the divining rod reports conclusions favorable to the dowsing hypothesis.

The present interest in the subject may be seen from the fact that there are societies in various European countries, at least, that are formed for the sole purpose of investigating dowsing.

It is interesting to note the theories that have been advanced to

account for the movement of the rod. Among the earliest was the idea that the motion of the rod was due to demoniac influence. Another was that there is a natural affinity between "things vegetable and things mineral" which accounted for the rod's dipping over veins of ore and water. Another assumed that water and other such substances give out emanations or "corpuscles" which rise from the minerals and enter into the rod, causing it to bow down "in order to render it parallel to the vertical lines which the effluvia describe in their rise" (**3**, p. 249).

In modern times it is pretty generally agreed that unconscious muscular action is responsible. (This theory was first advanced in 1660 by A. Kircher and G. Schott.) But there is disagreement when we try to decide what furnishes the stimulus. The most popular explanation among the dowsers themselves appears to be that of electricity. Even up to the present day it persists in the form of a theory that electromagnetic radiations produced by underground water affect the nervous system of the dowser and make him aware of its presence. One of the more recent proponents of this view is a Dutch geologist, Dr. S. W. Tromp (**9**) whose own experiments led him to favor a physical interpretation of dowsing.

We find, however, that this theory becomes inadequate when it is applied to all the dowsing results. How, according to a physical theory, can one explain the alleged location of an object merely by using a map or diagram of the area in which it is hidden? Why is it that the rod works equally well with so many diverse materials and objects? How can one expect to separate the electromagnetic influence of one object from that of another? Malebranche, the French philosopher, pointed out 250 years ago that if underground water produces a physical effect upon the dowser's nervous system, how much more of an effect should be produced by *exposed* running water; yet dowsers are not affected by this. It must be remembered also that dowsers are not usually affected by underground water while "off duty."

In the light of the facts so far available, the physical theory of dowsing appears to be ruled out. One is increasingly led to the view that the dowser may be a person with special ESP ability. By

clairvoyance he becomes unconsciously aware that he is over water, and automatic muscular action explains the rest.

* * * * *

It is best to look at dowsing against the background of its history. The use of the divining rod for discovering the presence of hidden substances or objects dates at least from the fifteenth century and probably originated among the German miners in the Harz Mountains, who used it to locate veins of ore.

The earliest mention of it is in a mine surveyor's manuscript written about 1430 in which a mining rod is alluded to. It is of passing interest to note that Martin Luther included the use of the divining rod in his list of acts that break the first commandment. The first really detailed account of its use (in this case for locating minerals) is contained in Georgius Agricola's book, *De Re Metallica,* published about 1546.

Not until we come to the seventeenth century do we find the divining rod used for discovering underground water. The Baroness Beau-Soleil, wife of a prominent French mining authority, became interested in the rod and carried out what, according to Barrett and Besterman (**3,** p. 9) may be said to be the first systematic experiments in dowsing. She was interested in using the rod to discover both underground minerals and water. Later she and her husband were charged with sorcery and put in prison, where they died about 1645.

Not many years after this, in 1678, the Jesuit, Dechales, in his *Mundus Mathematicus* referred to the use of the rod for finding water, and also gave a description of it.

It will be seen that the first dowsing on record was for minerals. Then came water divining, and most of the work of dowsers up to the present time has consisted in locating water supplies. But dowsers have not limited themselves to minerals and water.

In 1692, an incident occurred in France which broadened the scope of the rod to include its use in matters of morals. A murder was committed in southern France, and there were no clues to the murderer. Through the use of his rod, a peasant named Jacques Aymar allegedly traced and identified the criminals, who confessed

and were put to death. Aymar was the first outstanding dowser of whom we have knowledge.

Other uses of the rod have included finding buried treasure, indicating diseased organs in the body, finding lost underground passages, locating leaks in pipes, and even indicating the sex of an unborn child.

One of the most successful dowsers in the history of the divining rod was Barthélmy Bleton, another Frenchman, who was born about 1750. His alleged powers were investigated by a distinguished French physician, Dr. Thouvenel, who conducted some six hundred experiments with him and published all his findings. Bleton was highly successful in locating well sites and in tracing pipes buried underground. Like many dowsers, he professed to feel certain bodily sensations, described as a convulsive spasm affecting the pulse, on coming over underground water. He was subsequently "proved" to be a charlatan and dowsing a fake by its being demonstrated that a rod such as he used could be moved by almost imperceptible muscular effort and that the rod would not turn if Bleton erroneously thought he was insulated from the ground. In reality, the only thing proved by these facts was the shallowness of the thinking of those who asserted them as proof of roguery. Bleton's successes remain unaltered by them. They merely show the role of unconscious muscular activity and suggestion in the operations of the dowser.

There is another Frenchman who should be mentioned in a history of dowsing, and that is the Abbé Paramelle. He lived during the close of the eighteenth century and the first half of the nineteenth. This man did not call himself a dowser and did not use a divining rod. In fact, he looked with contempt upon such practices and explained his own great success at water finding in rational terms of careful observation of geological facts. He even published a book explaining his methods, but although it has been widely read, no hydrologist or water finder has succeeded in approaching his high level of success. Barrett and Besterman (**3,** p. 54) conclude that Paramelle possessed the same psychic gift as the avowed dowser.

Some of the more recent outstanding users of the divining rod have been Englishmen. There are four professional dowsers who

deserve special mention because of their large number of successes. In the order of fame they are: John Mullins, William Lawrence, William Stone, and Benjamin Tompkins. All of these men lived during the period from about 1810 to the beginning of the twentieth century.

Mullins seemed especially outstanding in his ability to locate water. His policy was "no water, no pay," and his rate of success shows he could well afford to make such a proposition. In case after case he was reported to have been spectacularly successful.

* * * * *

It is important to consider the more "evidential" cases as reviewed chiefly by Barrett and Besterman. The mere quantity of the data reported is impressive. Before dowsing can be established as a fact, however, certain counterhypotheses must be met.

It is argued that the results obtained by the dowser are due to his "practiced eye," to his being familiar with the locality, to his being guided by cues given consciously or unconsciously by others who are aware of the circumstances, to the fact that water can be found anywhere in certain localities, and to coincidence. Undoubtedly these arguments have been the explanation of many cases, but if we sift out only the cases which stand up best and then carefully consider these, we find them not easily explained away.

In judging the merits of an alleged case of dowsing, one must have at least some knowledge of the geological distribution of underground water. Dowsers usually tend to think of such water as running in underground streams and profess to trace these streams for long distances. In actuality, water usually exists in sheets of water-bearing strata. When a well is sunk below the water layer or impermeable stratum the water percolates into it. A spring or well depends for its supply upon the extent of the underground reservoir furnished by the porous stratum.

Nevertheless, in fairness to the dowser, as Barrett points out (1, p. 263), it should be mentioned that water *does* sometimes circulate underground, depending on the dip of the impermeable strata. It also flows in ducts created by faults and fissures in the rock. In all events, it can easily be seen that if a case is to be considered as evi-

dence of a "dowsing faculty," the geological facts must not be overlooked.

Barrett sought the advice of several good geologists and even had them criticize the cases he had collected. As is to be expected, geologists as a rule are highly critical of, if not antagonistic to, the claims of dowsers. While a few of the geologists frankly stated that they could not see how the dowsers had obtained their results, most of them suggested ways in which the results might be explained on the basis of sensory cues or geological knowledge. Barrett, however, points out that hind knowledge is something anyone can have. These geologists were giving their opinions after they "knew the score."

In assuming that a dowser's results can be explained in terms of a "practiced eye" or geological knowledge, the geologists sometimes endow the water diviner with more scientific knowledge than they themselves possess, for in quite a few instances a dowser was successful after a geologist had failed to find water. A case of this nature will be given later.

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There are certain types of cases which seem more clearly to favor the dowsing hypothesis, and these may be listed as follows: (1) Successful wells are located by dowsers near dry wells that are deeper. (2) The dowser succeeds where a geologist has failed. (3) The dowser accurately determines the depth of the water below the surface, especially in cases in which geological advice is contrary. (4) The same spot is located by two or more independent dowsers or by a dowser blindfolded. (5) Dowsing experiments are carried out in which a well is sunk at one spot where the dowser has indicated water and another sunk at a different spot where he has indicated "no water." (6) The dowser accurately determines whether or not water is flowing in pipes. (7) Lost or hidden objects are located.

Most of Barrett and Besterman's 335-page book is devoted to a discussion of cases which fall for the most part within the categories given above. These cases, as has been mentioned, are well documented and are given in detail. In order to show their calibre it will be useful to discuss a few of them at some length. To do

this I shall take each category in turn and discuss in detail a representative case falling under that heading. In some instances a single case will properly belong in several categories.

The first, it will be recalled, was that of a successful well located by a dowser near a deeper but unsuccessful well. (Barrett considered this type of case as especially good evidence for dowsing.) The instance I shall use in illustrating it is by no means the most spectacular, but I have selected it because a British geologist, after considering many of the cases, concluded that this was one he could not explain in terms of geological knowledge. (Of course, those cases for which he attempted to give explanations may very probably be just as good evidence, for this geologist was criticizing them *after* he knew all the circumstances.) The case is as follows (**3,** p. 144):

A Mr. T. W. Pickard, steward of an estate in Sussex known as the Toy Farm, recorded this instance which occurred about 1893. The dowser was Benjamin Tompkins, and the geologist who subsequently visited the location to make his own observations was Mr. Ernest Westlake, F.G.S.

An unsuccessful well had been sunk at the Toy Farm about thirty years before Tompkins was employed. This well was 167 feet deep but never contained any water and had finally been covered over. In 1893 Tompkins was engaged to locate a water supply. He was unfamiliar with the estate and was given no information which might have aided him in locating a likely spot. He walked over the ground with his forked twig until he had found a spot 450 feet from the old well and on ground about 20 feet higher. He estimated the water as being 70 feet below the surface. After finding this site, Tompkins was asked to dowse in the vicinity of the old covered-up well of which he had no knowledge. He crossed it several times but his rod made no indication. When he was informed that he had passed over an old empty well and asked how he thought it possible to obtain water at such a short distance from it, he replied that the old well was out of the run of the spring he had found. A well was dug at the new site and at 70 feet the first indication of water was noticed. It was not until it had been sunk to 118 feet,

however, that a good supply was reached. Later the well was deepened to 144 feet but with no increase in supply.

Westlake, the geologist, in commenting on the case, says the wells were sunk in chalk. He can see no reason for there being water in the new well but none in the old. He assumes that the success of the new well is due to the fact that water in chalk usually flows in fissures and the dowser successfully located one of these. Surface indications, to his mind, could not account for this success.

Many other cases could be mentioned here, some in which the dowser's well was sunk within a few feet of the old well.

The next category involves wells successfully located by dowsers after geological advice has failed. A case in point is that of the Waterford experiment (**1,** p. 106), which Barrett considers especially strong. John Mullins was the dowser involved, and G. H. Kinahan, at that time senior geologist to the Irish Geological Survey, was the geologist.

A bacon-curing establishment at Waterford in Ireland needed a large water supply and employed professional well sinkers to locate and bore a well for them. After two unsuccessful borings, one 292 feet deep and the other 1,011 feet deep, Mr. Kinahan, the geologist, was called in. He selected a spot which at a depth of 48 feet yielded a small quantity of water—not a tenth of the amount needed. He advised continuing the boring, and at eleven feet deeper the yield was even less. Mr. Kinahan then advised them to give up boring.

At this point Mullins was sent for. He was met at the station by a clerk so that he had no opportunity to find out about the situation. With his divining rod he located a site and said that at a depth of 80 to 90 feet a supply of water amounting to at least 1,500 gallons per hour would be found. When the boring was made, water in excess of 1,670 gallons per hour was obtained at a depth of 79 feet from the surface.

In discussing the case, Kinahan remarks: "As far as actual results went, I failed and the diviner 'wiped my eye'" (**3,** p. 85). He writes that the concern's premises (which were about 700 feet long) were over hard strata and probably were crossed by three water lines. Unless the bore hole was exactly on one of these water

lines a bore was useless. Mullins evidently placed his site successfully on a line, after many costly failures on the part of persons who, one would expect, should know most about such things.

Category number three contains the following type of case: the accurate determination of the depth of water below the surface, especially in cases in which geological advice was contrary. The Waterford case just discussed falls in this category to some extent. Mullins was correct in his estimation of the depth at which water was found. But the following case (**3,** p. 137) more properly belongs here.

A large hall at Uppingham, Rutlandshire, was standing empty because of the lack of a water supply. A Major Codrington wished to buy the hall but only if water could be found nearby. William Stone was called in, as well as a geologist who declined to be named. Stone recommended sinking a well at a particular spot where he said water would be reached at about 60 feet. The geologist said that water could be found anywhere in that locality at 20 feet, but that if the Major found none at that depth he would not reach any within 150 feet. A well was sunk at Stone's site. No water was reached at 20 feet, but on boring down to 66 feet, a supply of water was found which resulted in Major Codrington's purchasing the hall.

The next case (**2,** p. 144) falls under the headings of both the fourth and fifth categories: a dowsing experiment was carried out in which a well was sunk at one spot where two dowsers independently indicated water, and another sunk at another spot where they indicated "no water." Barrett himself supervised this experiment, which was conducted in 1899.

Knowing the precautions necessary to observe in order to obtain satisfactory evidence one way or the other, Barrett was very careful in his selection of a locality in which to try the experiment. The place he selected was by Carrigoona Mountain near Bray in Ireland. The district is geologically suited to such a test because it is neither largely waterbearing nor waterless.

The first dowser in the case was William Stone who had no idea before the day of the test where it would be held, and he said he had never been in that particular county before. He was taken directly to the location by Barrett and a Mr. Lefroy, a geologist

friend of Barrett's who took independent notes. Barrett himself was ignorant of the likeliest spots for obtaining water, so there was no possibility of his giving the dowser unconscious clues.

Stone located several places in a line where he said water would be found at less than fifteen feet, and he also indicated places where little, if any, water was present. These spots were marked. About three weeks later, an amateur English dowser, J. H. Jones, who likewise had never been in the district, agreed to try his hand in the same locality. He knew nothing of the marks made by Stone, and these had been carefully removed. He was given no chance to talk to anyone concerning the situation and was allowed to go over the ground alone so that no unconscious cues should be given. Jones indicated the same spots that Stone had marked, agreeing both with those where water would be found and where it would not.

Five borings were made in July, two on spots where the dowsers had indicated water, and three where they said no water would be found. The results were consistent with the dowsers' claims and even in the following November, at the final checkup, the results were these: The two *plus* bore holes were 20 feet and 22 feet deep. In the first, the water was standing 6 feet from the ground level; in the second, 6 feet, 3 inches. The three *minus* bore holes were 15 feet; 11 feet, 6 inches; and 16 feet deep. Of these holes, the first two were quite dry, and the last one had four or five inches of water in it. It will be noted that the two plus bore holes were deeper than the three minus holes. However, water was struck in the case of the plus borings only 8 and 6 feet, respectively, below the surface, and bedrock was never reached. The minus holes were abandoned in each case only after bedrock was encountered. The Carrigoona experiment, then, appears to be a case for the dowser.

Barrett asked two geologists their opinions on the foregoing case. Both thought it could possibly be explained by exceptional powers of observation and geological knowledge on the part of the dowsers, and they proceeded to show how a geologist could have produced the same results. Their explanations, however, do not adequately take into account the fact that the dowsers did not have any opportunity to gain knowledge of the district beforehand and that, furthermore, they did not show any signs of actively observing

their surroundings. One of the geologists admits: "It is very improbable—at least very difficult to understand—that a merely casual visitor to the place, however shrewd, if not versed in geological facts, could divine the existence of water in the field by cursory inspection, much less indicate with any accuracy the spot and depth at which it could be found" (**2**, p. 159).

We come now to experiments in which dowsers have attempted to tell whether or not water was flowing in pipes. This experiment was one tried with Bleton. He correctly traced underground pipes when there was water flowing through them, but when the water was secretly turned off, he was unable to follow them until it was again turned on (**3**, p. 39). There are several similar cases in which Mullin's rod indicated water running through pipes but failed to move over pipes in which no water ran. A more controlled experiment along this line will be given in detail later on.

The seventh category involves cases in which lost or hidden objects are located by a dowser. The tracing of underground pipes, already mentioned, is a common practice of dowsers. There are accounts of the location, in this way, of lost jewelry or money. A typical case is the following episode reported by a Mr. Bruce of Gloucester (**3**, p. 257): "Mullins also found a half-sovereign I had buried in a walk we were then making. I would have lost the half-sovereign if it had not been for Mullins, as I was so careful not to put any mark [on the place where it had been hidden] that I was not able to find the place myself, and when Mullins stopped and said it was under his foot, I thought he was wrong, but there it was!"

A few experiments under somewhat controlled conditions have been carried out in which dowsers' powers to locate hidden objects have been tested. Barrett supervised one such experiment in 1909 (**3**, p. 257). Three dowsers took part in it and a number of scientists were present. A dowser was sent out of the room, accompanied by a guardian, and while all the others who remained looked out the window, Barrett or another person hid a coin under a covering on one of the forty-five chairs in the room, all similarly covered. The person who hid the coin then went out of the room (so as not to give unconscious cues) and the dowser was called in. Out of five

trials, the dowser located the correct chair on the first attempt four times, while getting it correct on the second attempt once. In the latter case, the person hiding the coin admitted thinking of hiding it under the chair first guessed by the dowser but had changed his mind. Barrett and Besterman give the odds against chance for the results of this experiment as eighty million to one. From the account given by the authors, it appears that the selection of the chair under which the coin was to be hidden was left entirely to the whim of the hider. This fact might greatly affect the probability of success; hence, nothing can be concluded from this test. It serves, however, to illustrate the type of experiment that should be most actively followed up, for it can be easily controlled and mathematically evaluated.

* * * * *

The cases just described give a representative picture of the evidence for dowsing up to the present time. They are impressive and leave one with the feeling that this field is undoubtedly a legitimate one for parapsychology. In spite of their impressive nature, however, these cases cannot give one the same feeling of satisfaction regarding them as one feels toward results obtained under strict experimental conditions. There is a great need for an experimental attack on the problem, and while little data have as yet been collected by this method, the conditions are not difficult to arrange. As Rhine has put it, "we want to reduce the essential act of dowsing to as simple a test as possible so long as we are completely fair to the dowser and to the dowsing hypothesis."[1]

There is a belief among some dowsers that the rod will move over a pipe only if water is running in it. A simple experiment, then, can be based on the idea of having a dowser pass over a pipe with his twig and attempt to tell whether or not water is running in it. The experimenter or his assistant, located out of sight and hearing of the dowser, should randomly determine whether or not the water be allowed to flow on any particular trial. It is, of course, very important that the dowser be entirely unable to gain sensory cues which might guide him in his choice.

A brief, exploratory test was recently carried out by Rhine along

[1] From personal correspondence. Quoted by permission.

these lines. The dowser, a Mr. A., tried to tell whether or not the water in an underground pipe was flowing. Mr. A. was a business man of about sixty years, and not an experienced dowser. The water was turned on and off in a garden hydrant which was concealed from Mr. A. by bushes. Only 40 trials could be obtained, four runs of 10 trials each. During the first two runs Mr. A. was at a distance of about 50 feet from the hydrant, but in the last two runs he was 120 feet away. Mr. A. got 24 hits, a deviation of four above mean chance expectation. It is of some interest that most of the hits fell rather consistently within the first five trials in each run. There were 16 hits out of 20 trials in the first half of the pooled runs, and only eight hits in the last half.

Such a short experiment as this can be only suggestive, but it does indicate a profitable line to be followed in studying dowsing. More adequate tests would provide for a method of randomly determining whether or not the water would be turned on in any trial, for greater distances between the dowser and the hydrant, and for other precautionary measures to prevent sensory cues.

In carrying out such experiments certain things must be kept in mind if the investigation is to have its best chance at success. First of all, it is important that the dowser have some measure of confidence that he can distinguish flowing from standing water in an underground pipe. A little "indoctrination" or a few preliminary trials during which the dowser knows that water is on or off and can observe the action of his rod under both conditions may serve to build up this confidence. It is best to go about the test in a casual way, avoiding as far as possible distracting influences such as spectators. Experimenters in parapsychology have often found in conducting laboratory tests that a stage-fright effect, adverse to high scoring, is produced when subjects are initiated into an unfamiliar experiment. This should be kept in mind during dowsing tests.

Finally, a dowser's idea about his ability may or may not be correct, but it seems certain that it has a lot to do with his success. In testing a dowser, then, it is best to let him keep his notions and to proceed at his own rate, as long as this does not jeopardize the scientific aspects of the case.

What, now, should we conclude from a survey of the field of

dowsing? The undeniable fact is that the vast collection of cases makes a strong argument in favor of the dowsing hypothesis and strongly suggests that some persons have the ability to locate hidden objects, particularly underground water, by extrasensory means. Nevertheless, we cannot regard this as an established fact until rigorous scientific tests have ruled out all possible counterhypotheses. A beginning has already been made in the right direction and with continued research we may hope to settle the question of dowsing with finality.

When we consider the practical importance of this problem, added to its theoretical and scientific significance, we can hardly believe this challenging area of research will continue to go begging for explorers.

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RESTRICTED AREAS OF SUCCESS IN PK TESTS

By J. G. Pratt

ABSTRACT: That there is an amazing waste of effort in most of the trials in a dice-throwing test of PK is again emphatically brought home to us in this further study by Dr. Pratt. In this analysis of the Gibson Machine Series, he finds a still smaller portion of the record sheet showing a successful rate of scoring than was the case in his earlier studies. Out of the 72 positions in the set (three columns of 24 trials each), only two, taking the series as a whole, show significant success. These are the first and last trials in the first column of the set. In asking why the other 70 trials showed only chance results, Dr. Pratt brings up one of the most important problems in parapsychology. In spite of the differences in method, the comparison of this machine-thrown series and the earlier Gibson cup-thrown series shows interesting similarities. The elusive, extremely delicate character of the psi process is emphatically confirmed.—Ed.

POSITION EFFECTS within the data of dice-throwing tests were discovered while the research was still primarily concerned with the problem of whether PK occurs, and they were submitted as having a crucial bearing upon this hypothesis. The strength of this special evidence depended largely upon the tests of statistical significance applied in evaluating it. Some procedure was required which would provide a completely objective and uniform treatment of all the experimental series available. The use of different analyses in assessing different batches of data was not permissible; if the investigators had done this, they would have invited the criticism that they had selected methods which gave results in accordance with their own wishes.

The "quarter distribution" analysis was devised to meet this need for a uniform statistical test of position effects (**6**). The QD method could be applied to all the series available for analysis, regardless of differences in record structure. It served to reduce the position effects in all the tests to a statistical common denominator. By so doing, it answered conclusively the question of the extrachance character of the hit patterns. These were found to be highly significant in many of the individual series as well as in all the series combined.

The nature of the position effect evidence, furthermore, was such

that it could not be explained in terms of any of the other counter-hypotheses, considered either singly or in combination. The hit patterns alone, therefore, were conclusive evidence that PK occurs.

As soon as the genuineness of position effects as a psi phenomenon was established, the general question of their psychological nature arose. The types of analysis which were required for establishing the phenomena threw some light upon this more advanced problem, and the series of PK reports in this JOURNAL have frequently discussed the question of hit patterns and their psychological significance.

In general, however, the psychological study of position effects calls for a somewhat different approach and a different type of analysis. Differences in record structure, which were deliberately submerged in the QD analysis, are important facts in this new stage of the investigation. Also, it is necessary to treat the work of each subject separately and to distinguish carefully between experimental conditions. In short, everything possible should be done to avoid the risk of obscuring important psychological factors by the canceling-out effects of pooling of data until we are sure nothing is being overlooked by so doing.

Recently I started to re-examine some of the PK series which were known, from the results of the QD studies and of other analyses for position effects, to contain highly significant hit patterns within the record structure. Since the statistical significance of these findings was no longer in question, I did not hesitate to select those series for special study, as they seemed to offer the best hope of gaining new insights into the psychological factors involved. The first of the outstanding QD series chosen for detailed study was the work of subject L.H.G. in the Gibson Cup Series (**2, 4**), and this was followed by the M.P.R. High-Dice Series and Low-Dice Series (**3**).[1] The results showed that the relation between the record structure and success in scoring hits on the target was much closer than had previously been suspected. Also, there were indications of psychological factors operating with striking consistency in all three series. The nature of the findings definitely showed that the ap-

[1] The possibilities of this type of investigation had already been tested out by Woodruff and myself (**5**) in an exploratory study made upon a short series of our own PK records which had not previously been reported.

proach taken in these new studies was a fruitful one for the stated objective—that of trying to explain the nature of the position effects which were already known to exist.

Accordingly, this report deals with the re-examination of still another PK series which has already been shown to have significant hit patterning, the Gibson Machine Series (1). These results are of particular interest just now because they offer us an opportunity to compare the hit patterning of subject L.H.G. in the Machine Series with the findings from the earlier Cup Series, a comparison which is all the more interesting because of the differences in test conditions and record structure involved.

The Gibson Machine Series

L.H.G. served as the principal subject in the Gibson Machine Series, all of her work being witnessed and the results recorded by the experimenter, E.P.G. Three dice were thrown at a time in a mechanically rotated cage, and the subject's task was to try to influence them mentally to come to rest, at the end of each half-revolution of the cage, with a designated target face uppermost.

The record of each trial consisted simply of the number of target faces observed. One of the record sheets has been reproduced in Figure 1. It will be seen there that the results were recorded in columns showing the scores for 24 throws and that a set consists of three successive columns thrown for the same target face. There were always six sets on the page, one for each face of the die as target.

This series has already been reported from the point of view of the evidence it contributed for the occurrence of PK (1). The series was significant, both in its total score and in its position effects. All subjects together did 1,491 runs, with a positive deviation from "chance" of 191 hits. The CR is 2.71. L.H.G.'s results alone comprise 630 runs, with a positive deviation of 138 hits, the CR being 3.01. Thus her work accounts almost entirely for the significant total score of the series.

The position effects found were consistent with those of other experimental series. There was a horizontal decline in scoring between the left and right halves of the page. A comparable horizontal

decline was found through the three columns of the set. A vertical decline occurred between the upper and lower halves of the column. Finally, typical QD's of the page and of the set were found; that is, the greatest drop in scoring took place between the upper left and

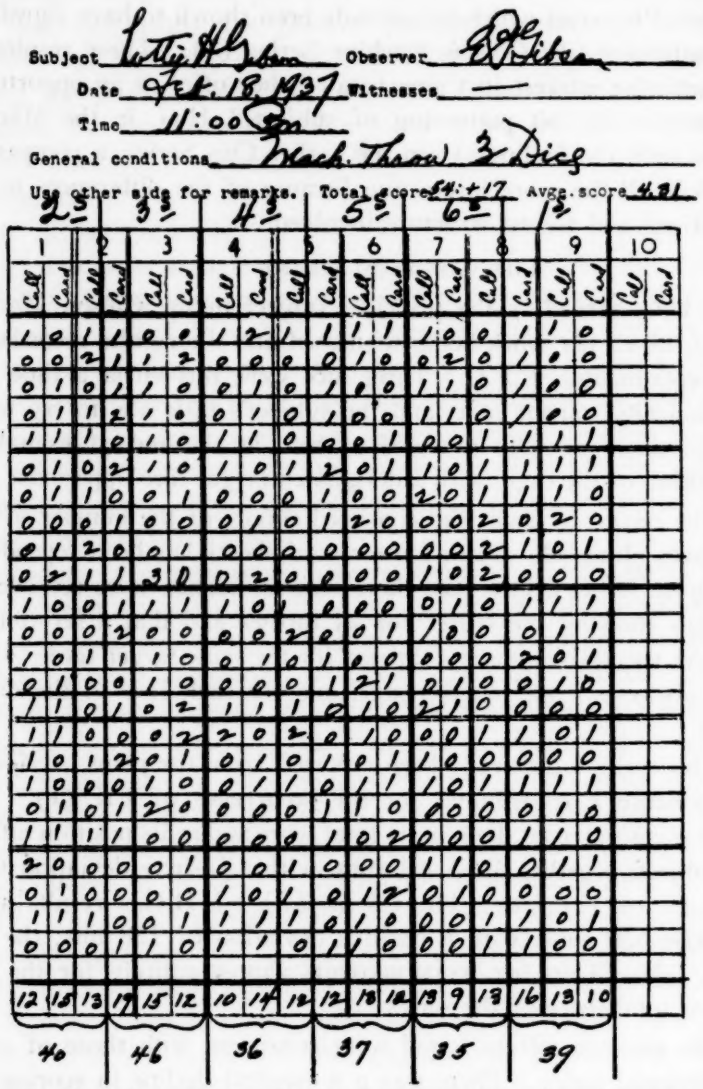
Subject Betty H. Gibson
Observer B.H. Gibson
Date Feb. 18, 1937
Witnesses —
Time 11:00 Eve
General conditions Mach. Throw 3 Dice
Use other side for remarks.
Total score 64: +17
Avge. score 4.21
1 2 3 4 5 6 7 8 9 10
Cell Card
40 46 36 37 35 39

FIG. 1. Typical record page showing the column, set, and page structure of the data in the Gibson Machine Series.

the lower right quarters in each analysis. The QD of the set was particularly significant, the CR of the difference between the first and fourth quarters being 3.15.

The figures quoted in this section are taken directly from the original report, and they include one page of results by L.H.G. in which only two columns were thrown for each target face. There are 11 of the record pages on which the usual three-column sets were used. Because of the greater convenience of working with a uniform set structure, the sheet of two-column sets is omitted from the present study.

Results

Hit Pattern Within the Set

The position effects which have previously been reported for this series suggest that the most interesting unit of data for further study, from the psychological point of view, is the set. It was in the set that the most significant results were obtained when the QD analysis was applied to the pooled data of all the subjects.

Since the present study will deal entirely with the results of L.H.G.'s work, I first applied the QD analysis to her sets alone to see if the position effects were significant when measured by this standard procedure. The results for the 66 three-column sets with which this study will deal are shown below. (The middle column of each set was omitted according to established practice.)

QD of Set for L.H.G.

99 +71 (1)	99 +3 (3)
99 +24 (2)	99 +7 (4)

$CR_{d(1-4)} = 2.49$

This result shows the expected decline from the first to the fourth quarters, and this is still significant in spite of the smaller number

of runs involved. We are justified, therefore, in singling out the pooled set of her data for closer scrutiny to see if we can learn anything further about the hit patterning which the QD result shows, in a general way, to be present.

To ascertain in detail the distribution of successes within the record structure, I pooled the 66 sets of L.H.G.'s data so as to keep

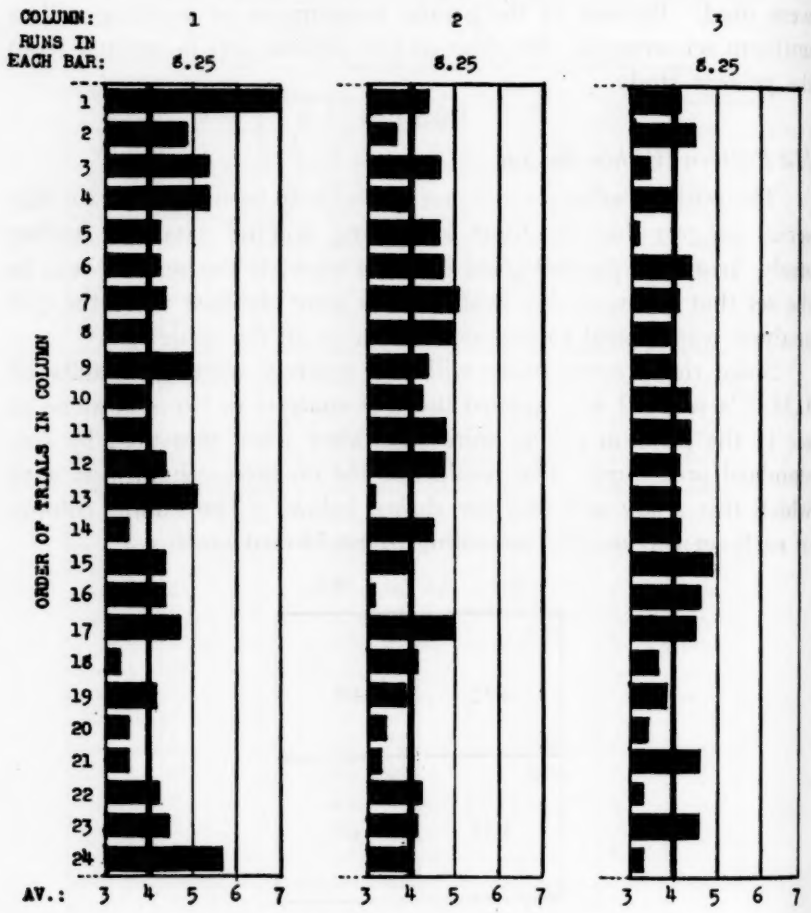

FIG. 2. Composite set of L.H.G. data from the Gibson Machine Series. This was derived by totaling and averaging the hits obtained in each of the 72 trial positions for all 66 of her three-column sets.

each trial position separate. The pooled set thus showed the score totals for each of the 24 throws in each of the three columns.

The results, shown in Figure 2 in terms of averages per 24 die observations, indicate that the outstanding success of the subject was highly localized within the set structure. The greatest number of hits was obtained on the first trial of the first column, and the next best throwing position was the last trial of the first column. Each pooled trial position represents 198 single die observations, or 8.25 runs of 24 dice each. The number of "hits" expected most frequently from this number of dice falling randomly would be 33. On the first and last trials of the first column of the set there were 58 hits and 47 hits, respectively. The SD of each trial position score is 5.24. The positive deviation of 25 hits on the first position thus gives a CR of 4.77, and that of 14 on the last trial, a CR of 2.67. Each of these selected trials therefore produced significant scores, and they were the only ones in the pooled set to do so. The next highest scores were made on the third and fourth trials of the first column, each of which gave a positive deviation of 11 hits and a CR of 2.10.

The different faces of the die were used as the target an equal number of times. In this situation mean chance expectation is for one-sixth of the dice thrown to fall with the target face uppermost, regardless of any physical bias of the dice. If a bias factor was present, it would have the effect of reducing the variance of the scores below that assumed on the binomial hypothesis (**7**). Any error from faulty dice entering into the above evaluations would therefore be on the safe side.

We knew to begin with that there were significant position effects in this pooled set. Also, we would have predicted on the basis of previous studies (**2, 3**) that the highest rate of success would be found in the first trial of the first column. Similarly, the last trial of that column was a very likely place for scoring salience to show up. But simply to show that each of these trials was significant is not enough to prove that they are the special locus of position effects within these data. To show that they represent significant hit patterning, it is necessary to test whether the scoring in these trials is significantly different from the remainder of the set.

The average of each of these two trials was found to be significantly higher than the combined average of the 70 other trials of the set, the CR of the difference being 4.45 for the first trial and 2.36 for the last trial of the first column. When the average of both of these salient trials combined in relation to the average of the 70 other trials was evaluated in terms of the binomial hypothesis, the CR of the difference was found to be 4.80. As explained above, any possible effect of dice bias upon this result based upon equal use of all the faces would be on the safe side. There seems to be little doubt, therefore, that the significant position effects in this series were contributed largely, if not entirely, by the outstanding scoring in only two out of the total of 72 trial positions in the set.

Other Findings

There are two reasons why the number of analyses made in the earlier investigations to discover possible psychological factors underlying hit patterning cannot be made with the same degree of conclusiveness in this series. One is that this series is, comparatively speaking, a small one in terms of the total number of runs involved. Comparisons between pooled sets for different sections of the data are therefore likely to be based upon such small numbers of individual trials that relatively large average variations might enter the picture on a purely random basis. In order to offset this difficulty to some degree, I decided to combine the two salient trials of the set into a single score and to contrast this with the rest of the set as a basis of measuring and comparing position effects in further analyses.

The other reason is the fact that the targets were not randomly distributed in the order in which they were used on the record page. This makes it particularly difficult to distinguish between two possible factors: location of the sets on the page, and target preference. No one was interested in position effects at the time the tests were made, and the order of targets on the page had no relevance for the objective of the experiment. The sole purpose was to see if a significant total score could be obtained under carefully safeguarded conditions *which excluded the possibility of dice bias.* The use of each target face for the same number of throws met these condi-

tions, and the experimenter was fully justified in permitting the subject to choose the target sequence on each page.

It is evident from the records that L.H.G. customarily chose the 6-5-4-3-2-1 order of targets on the page, as she followed this sequence on five of the 11 pages and departed from it only slightly in a sixth session. If the dice were biased, as we must suspect was the case in order to be on the safe side, the use of any particular target would not produce salience by virtue of this factor alone. When the pooled set does not contain an equal number of throws for each target, however, the average probability of scoring a hit would not be exactly one in six dice, and the variance of scores within the set would be affected in an unknown manner. Therefore, any evaluation of the statistical significance of the two salient trials should be based upon the *observed scores* within the particular pooled set in question.

The following empirical evaluation of position effects was therefore adopted: the 24 trial scores in each column were combined to give 12 scores by adding the first and twenty-fourth, the second and twenty-third, etc. This gave a total of 36 observations within each pooled set. The mean and the empirical SD of this statistical group were then obtained by the method appropriate for small samples, and the significance of the CR of the score from the two salient trials was determined by dividing the SD into its deviation from the mean. The CR obtained in this manner was independent of any possible dice bias.

Effect of Position of Set on the Page. The 11 sets which were first on each page were combined to give the pooled data for the first set on the page, and the sets were pooled in the same way for each of the other five set positions on the page. By the method of evaluation described above, the following CR's of the salient scores in the six sets across the page, respectively, were obtained: 2.41, 1.85, 2.84, 2.07, 2.24, and −1.26. These values show that the salient trials in the first and third set positions on the page are statistically significant within their respective sets, and the salience shown in the second, fourth, and fifth positions is, in each case, only slightly below the criterion of significance. In the sixth (last) set on the page, however, the (usually) salient trials actually score below

the average of the other trials of the same set. This result shows that the position effects which were characteristic of these data as a whole did *not* occur in the last set on the page. This evaluation of salience in relation to the position of the set on the page is based entirely upon the differences in *observed scores* within each set; hence the CR's are independent of any possible dice bias.

Because of the fact that the target order on the page was not random, further analyses were necessary to explain the decline in salience in the last set on the page. Was the psychological factor involved one of a critical change in motivation upon reaching this point in the page structure, or was the subject showing a target preference effect? To decide between these possibilities I isolated those sets in which the subject did *not* follow the 6-5-4-3-2-1 order of targets, and I found that the page distribution of salience was of the same character regardless of the target used. For example, three times the six-face was the target in the last set on the page, and these data also showed the total absence of salience which was characteristic of that position. Furthermore, there were three cases with the one-face as target in the first set on the page, and these were found to show the high degree of salience typical of the start of the page.

The evidence clearly indicates a relation between salience and position on the page. We may not be able to rule out completely the factor of target preference, but neither could it be established on the evidence from this series alone.

Analysis for Chronological Effects. The first work by L.H.G. in this series was done on February 13, 1937, and the last session was held on April 22 of that year. Each session consisted of a single page of data, one set for each target, as illustrated in Figure 1. The eleven pages of data, one for each session, should tell us, when considered in chronological order, whether the position effects underwent any systematic changes as the subject gained experience in the test situation.

An answer to this question may be obtained from a study of the score averages throughout the 11 sessions for the salient trials and for the nonsalient trials considered separately. These results are shown in Figure 3. We see there that high scoring was character-

istic of the two salient trials from the very start of the experiment. Furthermore, the level of success was relatively consistent throughout the entire series. The fourth session was the only one in which the (usually) salient trials gave a score below "chance." In view of the fact that these two trials together comprise only 36 die throws (1.5 runs) in each session, no special significance can be attached to this amount of variation in scoring.

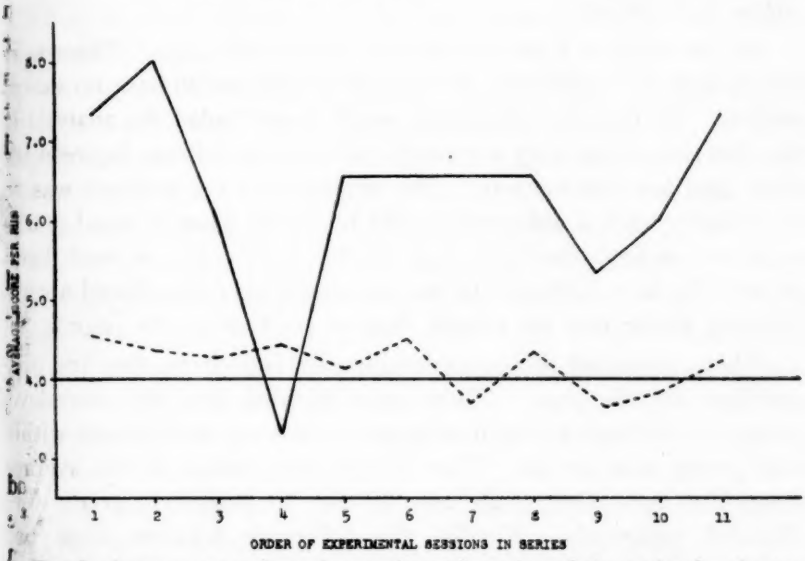

FIG. 3. Average score in each experimental session of the series for the two salient trials (solid line) and for the nonsalient trials (broken line).

When the first five and the last five sessions are compared, leaving out the middle page of the series to give equal groups, the two salient trials score a total of 47 hits in the former group and 48 hits in the latter. The results show that there was no significant change in rate of success from the start to the finish of the tests as far as the two outstanding trials of the set were concerned.

The chronological results of the nonsalient trials, on the other hand, show a general decline in score average through the series. In the first five sessions, these nonsalient trials (totaling 262.5 runs) give a positive deviation of 91 hits, with a CR of 3.08. In the last five sessions, however, the total score of the corresponding trials

gives a negative deviation of 13. The CR of the difference between these two groups of sessions is 2.49, for which P equals .006.

Score Level Effects. Are the position effects more prominent in high-scoring sets than in low-scoring ones? To answer this question I grouped the sets according to the total scores made and obtained the difference in average for the salient trials and the 70 other throws of each set. The mean chance expected score on a single set is 36 hits.

In this analysis I omitted the last set on each page. These were known both to be generally low in total scores and to have no salient scoring. To have included them would have loaded the analysis in the direction of showing a greater difference in salience between the high- and low-scoring sets. The objective of the analysis was to see whether such a difference would be found when it could not be explained on any other basis than the differences in score level themselves. To have included the last set would have introduced a complicating factor into the results, that of position on the page.

There remained for consideration 55 sets from the first five positions on the page. These were divided into six score-level groups on the basis of the total score in each set, and the sets within each group were pooled. Then within each pooled set the average scores for the salient trials and for the 70 nonsalient trials were obtained, separately. Finally, the difference between these two averages was found for each score-level group as a general indication of the degree of salience shown. The results for the six levels of general scoring success were as follows:

Set Scores	26-33	34-35	36-38	39-41	42-44	45-49
No. of Sets	9	9	9	10	10	8
Diff. in Average (Salient and non-salient trials)	2.94	1.12	1.23	4.50	2.50	6.21

The general trend shown by these figures is one of larger differences in average at the higher score levels. This result, on the strength of this series alone, can only be taken as suggestive of an increase in the prominence of position effects as the total score goes up.

Discussion

The position effects brought to light by this investigation are of interest chiefly because of the opportunity presented to make comparisons with the results from other studies, particularly those of the same subject in the Gibson Cup Series. It will be recalled that the structure of the set in the earlier series differed from this in two respects: in the earlier experiment each column was only six throws long, as compared to 24 here; and there were 10 columns in each set instead of three.

A further difference is the fact that the Cup Series contained both witnessed and unwitnessed trials, while all of L.H.G.'s work in the Machine Series was witnessed. The comparison between the witnessed section of the earlier series and the present results should be the more informative one to make in regard to the effects of the differences in record structure in the two test situations.

The general characteristics of the hit patterns in the two series have one important feature in common. Both show the outstanding rate of success on the first trial of the set. With the short column of the Cup Series, there was no salience of scoring in the last trial of the first column, such as occurred in the longer column of the Machine Series.

This similarity of results at the start of the column but not at the end suggests that for L.H.G. a six-trial column was somehow too short to show both beginning and ending salience. The longer 24-place column, on the other hand, may have given the last trial a distinctiveness that resulted in significant scoring.

It is interesting to speculate regarding what the results might have been if a column of intermediate length had been used. We know that subject M.P.R. produced a pronounced U-curve in the first column of the set when she made 12 throws to the column. This suggests that the length of column necessary to show both beginning and ending salience might be somewhere around 12 throws. On the basis of the limited data available, however, we may profitably generalize only to the extent necessary for setting up hypotheses for later testing. It seems quite likely that no general rule will be found to apply, but that comparison of subjects will

show that variations occur in the hit patterns even within identical record structures because of differences in personality factors.

The relation in L.H.G.'s data between hit patterns and the position of the set on the page is quite similar in the two series. Indeed, the findings are as nearly identical as the differences in page structure in the two test situations would permit. In the Cup Series each set extended half way across the page. Thus there was room for only two across the page, as contrasted with six in the Machine Series. At the same time, the short column in the Cup Series left room, vertically, for as many as three or four such pairs of sets on the page.

In both series, sets at the left-hand side of the page showed significant position effects, while those at the right-hand side did not. In the Cup Series there were no intervening sets in going across the page, and this rule, as stated, applied to the witnessed sets regardless of what level they occupied on the record sheet. In the Machine Series, with four intervening non-marginal sets across the page, the characteristic position effects were found to be present in about equal degree in all except the last one on the page.

In the work of M.P.R. there were three sets across the page in each half of the sheet, the upper half devoted to high-dice trials and the lower to low-dice. Her results agreed with those of L.H.G. in one respect: the sets at the left-hand margin of the page showed more pronounced position effects than did those at the right-hand margin. There was one point of difference: the decline in position effects occurred immediately after the first set on the page rather than just before the last one. In the study by Woodruff and myself there was a regular decline in effect in going across the page. All of these studies indicate that position on the page is a psychological factor in the production of hit patterns. That is, the psychological situation as it affects the subject's PK response apparently changes as he progresses across the page. In the Machine Series, it is not possible to tell within which larger unit, the page or the session, the salience of the set structure shows a significant decrease between the first and the last set position. A distinction between these two factors could be made within the M.P.R. data, however, and the location of the sets on the page was found to be the controlling factor.

Also, in the Cup Series each tier of sets on the page in the witnessed section showed the difference in salience in going across the page. The evidence is strong, therefore, that the psychological factor involved is one which is related to the record structure.

In the many batches of PK data which have been analyzed for position effects, none have been found with any significant chronological differences in the experiment. There have, however, been some instances in which suggestive declines of this nature have come to light.

In the Machine Series the results obtained from the chronological analysis of position effects showed that there was no lowering of average success in the salient trials of the set. Instead, there was a decline in scoring in the other 70 nonsalient trial positions. It seems as if there were inhibitory factors at work on these low-scoring throws, factors which were not completely successful in ruling out extrachance scoring in the first half of the experiment but which did reduce the success to the "chance" level in the second half. Other investigators (1) have suggested that the position effects in this series may be caused by an active inhibition of the psi function in the nonsalient trials of the record structure. The present investigation has produced suggestive evidence in support of this view. The significant scoring on the nonsalient trials at the start of the series and the decline in scoring *on these trials only* as the subject gained experience in the test situation looks like an active inhibition of PK at these points within the record structure.

The suggestive finding that the prominence of position effects is directly related to the general level of scoring success confirms previous results found in the work of L.H.G. and M.P.R. In the Pratt-Woodruff PK study a relation was also found, but the more pronounced position effects were in the lower-scoring columns. The consistency of these results seems to establish the fact that the psychological factors responsible for position effects in PK data vary in their effect from set to set, and that these variations are sufficiently marked to be reflected in the total score on the set.

Concluding Remarks

Again we have discovered, upon studying more closely the patterning of hits within the record structure of a PK series, that the

significant scoring is restricted to a small percentage of the trials. The Machine Series, indeed, has proved to be an example of even more restricted scoring than that found in the work of M.P.R., reported in the last issue of this JOURNAL. The scoring area compares closely with that of L.H.G.'s own earlier witnessed work in the Cup Series. There the significant throwing was restricted almost entirely to the first trial in the set of 60 throws. Here we have found a significant number of hits only in the first and last trials of the first column of the set.

The Machine Series was launched right after the Cup Series was completed. The two situations presented to the subject what would appear to be quite different psychological tasks. In the Cup Series the subject took an active part in the tests, shaking the dice in a cup and rolling them out onto a blanketed surface, then picking them up for the next throw. In the Machine Series, she simply sat and directed her attention upon having the dice fall within the cage to get a high score. About the only features which the two situations had in common is the fact that the subject knew in each case what her results were and she was aware of the manner in which the records were being kept. Yet from these very different situations came almost identical hit patterns. There appear to have taken place changes in the subject's motivation which were so clearly and so closely related to the structure of the task to be performed that we cannot escape the conclusion that these effects are fundamentally related to the subject's total psychological situation. If we can explain the position effects, we will undoubtedly have gone a long way toward explaining the PK process itself.

One of the most interesting relations discovered is the manner in which the subject's scoring suddenly dropped to insignificance after the first trial in the witnessed tests, whereas both L.H.G. and M.P.R. showed only a gradual decline in success within the first column of their unwitnessed tests. This suggests that the difference in the two social situations is important in determining how soon the inhibiting factors set off by the test procedure will take full effect. With or without a witness, the advantage of making a fresh start shows up in the high average success of the first trial. When the subject is working alone, this favorable situation carries over to

successive trials in the column, often enough, at least, to show up as a more gradual decline in average success. If the PK function is such a subtle and elusive one, however, that the presence of an observer, even a very sympathetic one, may cause the subject to inhibit the process immediately after the first trial, it is understandable that another kind of observer might interfere with success even on the first trial.

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A NEW DISPLACEMENT EFFECT IN ESP

By Edward Bindrim

ABSTRACT: Some subjects in ESP tests have been found to miss the target card (or other object) aimed at but to hit the card just ahead of or just following the target. This is called displacement. It is not always easy to measure this effect, especially when it appears in small traces and when the displacement shifts from one direction to the other, thus tending to disperse the effect. Mr. Bindrim has devised a new technique which he believes offers a more sensitive way of measuring this effect. It estimates the consistency of the displacement within each run.

In the work here reported he tried out his method on a series of ESP data that have previously been reported. It was a series of tests that did not give a significant total score on the intended target. Neither did it yield any significant evidence of displacement, forward or backward, when measured by the usual method. The new method, however, showed significant consistency of displacement, or, as Bindrim calls it, "displacement trend." As he had predicted, it was in the low-scoring runs only that the displacement trend was obtained; that is, in the runs with a score running from zero to three on the intended target.—Ed.

Introduction and Preliminary Study

History of Displacement

It was reported in 1940 by Whately Carington (1) that extrasensory perception can function by hitting targets other than those at which it is aimed. Using drawings as stimuli in a GESP test (general extrasensory perception allowing both telepathy and clairvoyance to take place), he found that his subjects' reproductions tended to resemble other drawings in the series more than the one designated as target. At Carington's suggestion, S. G. Soal (7) rechecked some of his own GESP card test records for comparable "displacement," as Carington called this effect, and he found that two of his subjects, instead of hitting the target card, tended to make calls which matched the card immediately before or after the actual target in the symbol sequence.

In collaboration with K. M. Goldney, Soal (8) conducted a new series of similar card tests with one of his displacement subjects, B.S., and they found that the phenomenon of displaced hitting continued to occur, particularly in the forward direction.

There have been other studies of the displacement effect. Although Carington's was the first signed article to report this effect, an anonymous physical scientist had presented results in this JOURNAL in 1938 which showed a strong tendency to hit the card coming just before or after the actual target, particularly in connection with low target scoring attributed to fatigue and illness (12). Following the publication of the researches of Carington and of Soal, Thouless (11) reported evidence of marginally significant displacement in the direction of hitting ahead of the target. On the other hand, Russell (4), in 1943, examined the records of eight outstanding ESP experiments already reported but found no significant evidence of displacement. More recently, however, Stuart encountered rather striking displacement evidence in ESP tests with pictures. In one of these, a clairvoyance experiment, displacement occurred in the backward direction; that is, the subject hit the immediately preceding stimulus (9). In another, an experiment made under GESP conditions, displacement in the forward direction was found (10).

The relation between displacement scoring and the rate of scoring on the designated target is of special interest. Humphrey (2) pointed out that Soal and Goldney's subject actually rejected the target to a significant extent in runs showing significant positive scoring on both forward and backward displacement. This occurred, however, only with a particular sender. But in a more general way, the same negative relation between target and displacement scoring was found by Humphrey in her study of Stuart's displacement data (3). Through her analysis of the form quality (expansiveness and compressiveness) of the response drawings she found that the compressive subjects in clairvoyance tests scored significantly *below* mean chance expectation on the target but significantly *above* on the just-previous stimulus.

From this brief review it appears, first, that displacement may be said to be moderately common in ESP records, and, second, that there is some indication that it may be associated with target rejection or scoring below mean chance expectation. The present study hinges on these two points. That is, it seemed reasonable to look for new evidence of displacement in ESP data, and it seemed some-

what more likely that it would be found in low-scoring records than in those with high scores. The displacement effect had been found both in drawing tests of ESP and in card test data, but since the greater bulk of the available test data are from the card procedures, it was decided to focus attention on the card-calling experiments.

Since displacement occurs in both forward and backward directions, however, a special modification of method was needed. The evidence indicated that both forward and backward displacement might be expected, and that even with the same subject it might shift its direction. With experimental data collected from many subjects, these differences might vary through all sorts of proportions. These variations of both forward and backward displacement, intermingled, would dilute the effect considerably and might easily obscure its presence. A more sensitive measure was needed, one that would detect a displacement trend in a single run and preserve it, no matter whether the next run or the next subject showed the opposite type of displacement.

Procedure for Scoring Displacement Trend

The method which was chosen was as follows: Each run of 25 ESP card trials was divided into top and bottom halves and each half was checked for both forward and backward displacement. To score forward displacement (frequently referred to as precognitive, or +1), the run was divided after the twelfth call, the twenty-fifth one being omitted because it cannot be scored for forward displacement. To score backward displacement (frequently called postcognitive, or −1), the division is made after the thirteenth call, the first call being omitted since it cannot be checked for backward displacement. The two displacement scores in each part of the run were then compared, and if the same *direction* of displacement, either forward or backward, gave the larger score in both halves, the run was said to show *displacement trend*. On the other hand, if one direction of displacement gave a higher score in one half and the other direction of displacement, a higher score in the other half, there was said to be absence of trend. Runs which gave identical forward and backward displacement scores in either half were left out of this analysis as there was no basis for saying that the run as a whole either did or did not show trend.

When we consider only those runs which can be evaluated for trend by this method, the mean chance expectation is for exactly half of the runs to show displacement trend. Any observed deviation from "chance" in the number of runs showing a consistency of displacement can readily be evaluated statistically.

Inasmuch as interest centered chiefly on the question of whether there was present in runs with low target scores any displacement effect which would be revealed by this method, I decided to apply the procedure to runs with target scores in the 0-3 range. I made a preliminary test of the procedure on 80 GESP runs collected from Finch Junior College students. Among these data, there were only 13 runs that could be evaluated by this method, but 10 of these had displacement trend.

Statistical Test of the Hypothesis

Data for the Test

On the basis of the encouraging preliminary results, I decided to test the hypothesis on a larger body of data. Dr. Gertrude Schmeidler was consulted, and she very kindly made available 1,494 runs from five experimental series made in connection with her sheep-goat comparison, a study of the role of attitude in ESP. Four of these series were group tests with students in college classes as subjects (**6**), and the fifth was composed of subjects who had been tested individually (**5**). In all the tests each subject participated in one session consisting of nine runs. Instead of the closed, five-by-five ESP deck, a random method of selecting the conventional symbols was employed. In this way a given symbol could be repeated any number of times in a run. These symbols were listed in nine runs on a key sheet. The key sheets were sealed in envelopes and a different list was used for each class tested and for each subject in the individual tests.

The methods for controlling the test against sensory cues, recording errors, and other counterhypotheses in the original ESP tests need not be described in detail here. The experimental conditions were adequately safeguarded, but in any case, the data had all been collected before the method of scoring displacement trend was devised.

General Analysis of Data by Established Methods

Before checking for displacement trend it is important to know the general results from these data when analyzed by existing methods, particularly as regards displacement. These results are given in Table 1. The scoring on the target is not significant nor is the forward or backward displacement. Even the runs in the 0-3 range show no significant deviation when checked for displacement. These data are therefore well suited for use in testing the sensitivity of the new method for measuring displacement by the consistency of its trend in the low target-scoring range.

Table 1

ANALYSIS OF THE SCHMEIDLER DATA FOR TARGET HITS AND DISPLACEMENT SCORING

Type of Analysis	Number of Runs Scored	Deviation	SD	CR
Target (All Runs)	1494.00	+ 33	77.30	.43
Forward Displacement (All Runs)	1434.24*	−167.2	75.74	2.21
Backward Displacement (All Runs)	1434.24*	− 1.2	75.74	.02
Forward Displacement (All Runs with 0-3 Target Hits)	331.20	− 60.0	36.40	1.65
Backward Displacement (All Runs with 0-3 Target Hits)	331.20	− 35.0	36.40	.93
Forward Displacement (Runs in 0-3 Score Range Which Could Be Checked for Displacement Trend)	203.52	− 31.6	28.53	1.11
Backward Displacement (Runs in 0-3 Score Range Which Could Be Checked for Displacement Trend)	203.52	+ 17.4	28.53	.61

* These figures are smaller than the total number of runs shown for the direct target analysis, for the reason that only 24 displacement calls can be checked in each run of 25 symbols. In checking forward displacement, the first card in the symbol order is omitted; in checking backward displacement, the last one is omitted.

Results of Displacement Trend Analysis

The Schmeidler data were found to contain a total of 345 runs with target scores of 0-3. These were checked for forward and backward displacement in each half of the run by the trend analysis method already described and used for the preliminary study.

There were 133 runs in which the forward and backward displacement scores were equal in either the top or the bottom half of the run, or in both. As explained earlier, these could not be evalu-

ated in terms of a consistently favored direction of displacement, and they were consequently omitted from the analysis for displacement trend.

In the remaining total of 212 runs, 124 showed displacement trend and 88 did not. Chance expectation would give an equal division between runs with and without displacement trend, and thus we get a deviation of 18 in the direction predicted on the basis of the preliminary study. The SD is 7.28 and the CR is 2.47, which is significant, with a probability of .007.

After the analysis for trend was completed, Dr. Schmeidler suggested that a correlation test of the same general effect could be applied to all 345 runs in the 0-3 range, and she kindly assisted in applying it to the data. This analysis consisted in obtaining a product-moment correlation between X and Y, X being the difference between forward and backward displacement in the first half of the run, and Y this difference in the second half. It was not necessary to exclude any runs in the 0-3 score range from this analysis, as even those runs which showed no difference between forward and backward displacement in either half of the run could be admitted to the correlation study. The correlation coefficient obtained was +.11, for which the probability is .02. Thus this method gives suggestive but not quite significant results when applied to these same low-scoring runs.

Findings Which Bear on the Nature of Displacement Trend

Relationship Between Displacement Trend and Run Score

In the preceding displacement trend analysis, runs with target scores of 0-3 had been selected because they seemed at the time to represent the range in which displacement was most likely to have occurred. After the trend analysis was completed on that range of target scoring, the remaining runs scoring four and above were analyzed by the same method for purposes of comparison and control. All the results were grouped according to target scores and the percentage of runs which showed displacement trend was determined for each group. These percentages, along with their respective CR's, are shown in Figure 1.

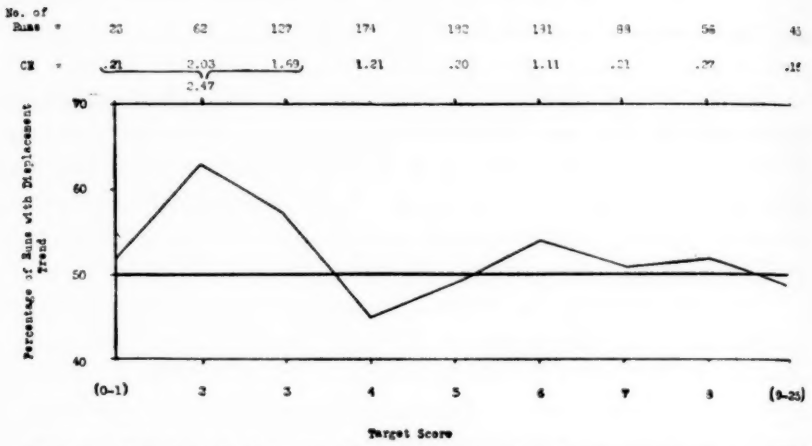

FIG. 1. Relationship between displacement trend and the target score. CR's and number of runs analyzed in each score group are shown above the graph. Unevaluable runs have been omitted.

The number of runs in each score group is shown at the top of the graph. No particular score level was significant in displacement trend. These results show that there is no evidence for displacement trend in these data outside the runs in the 0-3 target score range.

Displacement Trend and Run Scores in Each of the Five Experimental Series

We have just observed that displacement trend occurs only in the lower range of run scores on the target when the data are considered as a whole. The fact has been mentioned that the data analyzed were collected originally from five ESP series, four of which were group tests. It is interesting to note how these separate series contribute to the total effect of displacement trend shown in Figure 1. Each of the five groups was analyzed independently for the relationship between displacement trend and target score. In this analysis, runs with target scores of 0-3 were grouped, and also those of the 7-25 range, to provide groups of a size comparable to that of the 4, 5, and 6 target-score groups. In Figure 2, the five test groups are shown independently, with the number of runs in each indicated in the legend of the graph. Four out of five groups

follow the same pattern with only slight divergence throughout the entire range of target scores, with the exception of the runs in the 7-25 target range. This regularity of variation in four of the target score groups suggests that, in addition to the trend phenomenon found in the 0-3 range, there may be a related factor to look for in the variations found in the 4, 5, and 6 ranges. The lack of regularity in the 7-25 range is interesting in the light of the fact that Russell found no displacement in series with significantly high scoring on the target. While these observations are far from conclusive, they are indeed suggestive.

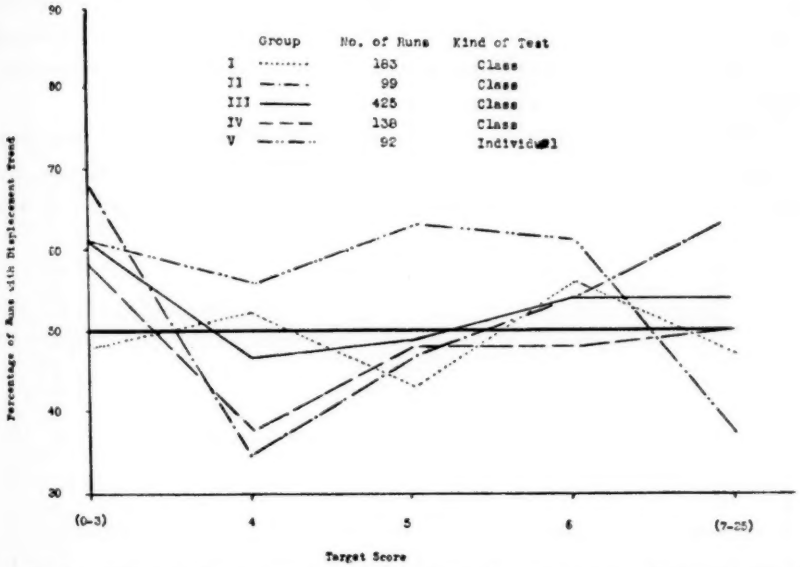

FIG. 2. Relationship between the percentage of runs with displacement trend and the target score for each of the five test groups. Unevaluable runs have been omitted.

Order of Runs on the Page

With regard to displacement trend in the runs with low target scores, it is interesting to know whether there are any position effects in the horizontal distribution of this phenomenon throughout the nine runs that each subject completed in the experimental session. The runs were divided into nine groups according to their respective positions in the session. These nine groups were then analyzed

to determine the average target score of the runs as well as the percentage of displacement trend in the 212 evaluable runs of the 0-3 range of target scores. The results are shown graphically in Figure 3, where the left-hand axis represents the target score and the right-hand axis, the percentage of trend in the low-scoring runs. The rank order correlation between percentage of displacement trend and average target score is +.36, which is not significant for nine pairs but which suggests a direct relationship between displacement trend and average target score. Since the percentage of trend is statistically independent of the number of runs scoring 0-3 in any given run position, this relationship may be interpreted as suggesting that the percentage of runs showing displacement trend will increase in run positions where the general target scoring average of the group of subjects increases.

This tendency is more readily observed when the runs are combined in sets of three corresponding to their positions across the page. The effect obtained by grouping the results for runs 1-3, 4-6,

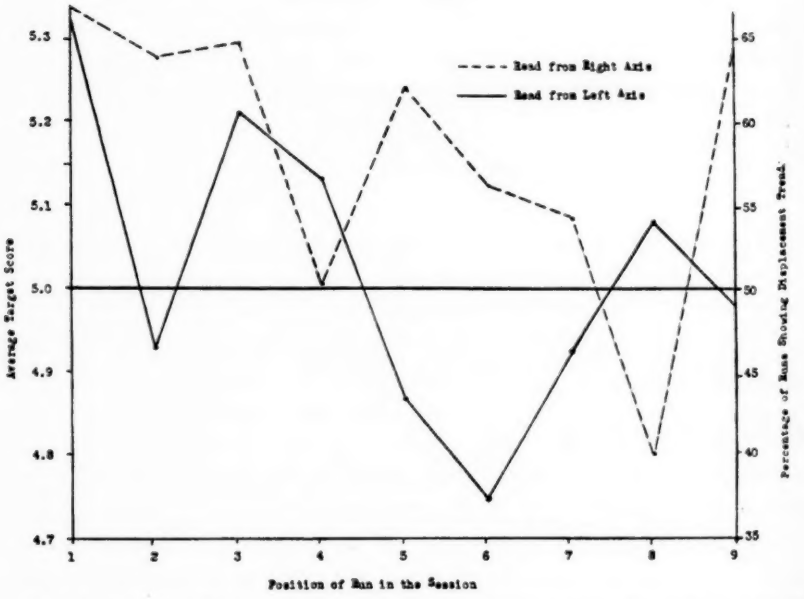

FIG. 3. The relationship between the average target score (solid line) for all runs and the percentage of runs with displacement trend in the 0-3 target score range (broken line) analyzed by run position.

and 7-9 (see Figure 4) is a general tendency for both displacement trend and target scoring to decrease in the course of the session. These results are not statistically significant, so any attempt at an explanation must remain highly speculative. However, it seems possible that they may have been produced by gradually declining ESP scoring in terms of both target hits and displacement trend through the three sets of the session. In this respect it is interesting to note that the total deviation for trend and target scoring in the first three run positions is almost twice as great as the total deviation for trend and target scoring in the middle three or in the last three run positions.

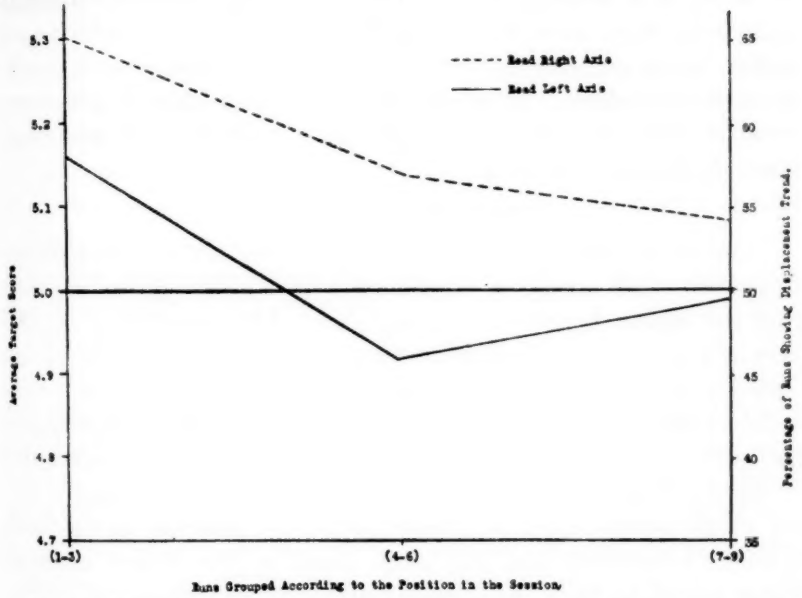

FIG. 4. The relationship between the average target score (solid line) and the percentage of displacement trend in runs of the 0-3 score range (broken line) when the runs are grouped in sets of three according to their position in the session.

Sheep-Goat Analysis of Runs with Displacement Trend

The runs under analysis have already been placed in the "sheep-goat" categories by Dr. Schmeidler who used this method to distinguish subjects who accepted the possibility of ESP under the conditions of the test (sheep) from those who rejected it (goats). The

212 low-scoring runs which were evaluated for displacement trend were divided into groups according to whether they were done by sheep or goats. The sheep contributed 61 runs with displacement trend and the goats 62. Another run with trend was done by an unclassified subject. Of the runs without trend, 43 were made by sheep and 45 by goats. The results show no relationship between displacement trend and the sheep and goat classifications.

The Direction of Displacement in Runs with Trend

The low-scoring runs with trend were studied to determine whether one particular direction of displacement, either forward or backward, was favored. Of the 124 low-scoring runs with displacement trend there were 58 runs with displacement trend which gave higher scores on forward displacement, and 66 that favored backward displacement. It would therefore seem that displacement trend in these data depends equally upon both forward and backward displacement scoring.

Discussion of Results

The displacement trend in the Schmeidler data is statistically significant, and it may be concluded with a fair degree of safety that the chance hypothesis is ruled out. The probability of .007 does not stand in need of correction since the findings are in line with the hypothesis formulated as a result of previous work. The small series of data from the Finch Junior College experiments had pointed the way to this finding. Furthermore, the expectation of finding displacement connected with low-scoring runs had grown out of the earlier work of Humphrey in her analysis of the Soal-Goldney experiment and her work based on the Stuart material. There seems to be no reasonable alternative hypothesis worthy of serious consideration.

Perhaps the most important aspect of the present study is the methodological one. A new method has been introduced for the study of displacement, a method evidently more sensitive for certain purposes than the procedure of evaluating total deviation, which has customarily been used. For the data analyzed, the total deviations of the displacement—either the forward or the backward displacement, or the two taken together—are not significant. The

greater sensitivity of the new technique depends on the fact that it measures the *consistency* of displacement in the run itself, which does not cancel out from run to run or subject to subject as may happen in the fluctuations of displacement scoring where total deviation is measured.

It may even be profitable to suggest that more attention be given in the general evaluation of psi score data to the measurement of consistency within small units. We know that psi capacities function both positively and negatively, responding to the target in the one case and rejecting it in the other without conscious awareness of which is occurring. The frequency of occurrence of significant negative scoring when the subjects were trying to hit the target suggests that a great deal of cancellation of effect occurs in psi experiments apart from the natural fluctuation of chance. Emphasis upon consistency may bring to light the operation of psi processes in data where their effects are at present obscured.

It must not be overlooked that by means of the analysis for displacement trend another large batch of ESP data, this Schmeidler series of 1,494 runs, which was insignificant in terms of its total score, has here been found to contain evidence of ESP when analyzed for consistency of displacement.

A curious and anomalous situation is found in the fact that even though displacement trend is associated with the runs having low target scores (range 0-3), it is also found to be associated with the area having the higher target scores when the position of displacement trend on the record page is considered. In other words, while displacement trend occurred *in* runs giving low target scoring, it appeared also *near* runs having high target scoring, which dual relationship suggests that the displacement trend and the higher scoring on the target require in general the same operational conditions (see Figure 4). On the one hand, there is an effect that shows up predominantly either as displacement trend or as target scoring, but not as both, in the individual run. To complete the picture, we find this effect in both its forms decreasing in amount as we proceed across the page.

Another interesting feature of the study lies in the fact that although the displacement trend was significant, the displacement

deviation was negative. It will be recalled that low scoring on the target was found by Humphrey to be associated with outstanding displacement scoring in the earlier work of Soal and Goldney and in that of Stuart. In the association of displacement with low-scoring target runs, the present study does bear out the expectation based on the Humphrey findings in those experimental series. But the surprising feature of the Schmeidler records is that there seems to be a general avoidance, not only of the target, but of the adjacent stimuli as well.

Yet again, we must recall that this spread of the rejection reaction does appear to be greatest in that part of the session or in that place on the record page in which the higher scoring runs are found. Table 1 indicates there is a negative deviation, particularly on the forward displacement, for the series as a whole. This deviation for forward displacement is almost significant (CR = 2.21). Interestingly enough, the negative deviation on the displacement scoring is porportionately somewhat greater for both directions on the 345 runs which scored 3 or below on the target.

In general, these results favor the view that displacement is not a motivated effect, but rather, one of cognitive disorientation, a confusion of aim, which is easily enough conceived in a form of perception so entirely unconscious. Some of the features which suggest this non-motivated character of the displacement may be mentioned. First, there was no appreciable difference in frequency of displacement trend between the sheep and the goats. Second, there was a fair amount of spread over the different sub-groups of subjects participating in the Schmeidler experiments. Third, it is somewhat difficult to imagine that it was ESP directed in accordance with the subject's aim which rejected the target stimulus in the low-scoring run, and at the same time gave the displacement trend. These neighboring stimuli were presumably not of interest either to the subject or the experimenter, and were not a part of the test program. Thus the rejection effect cannot well be supposed to be other than an accidental spread of negative ESP in action, a mere scatter of the avoidance reaction that produced the low scoring on the target (assuming, of course, that these were not chance results).

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BOOK REVIEWS

THE REACH OF THE MIND by J. B. Rhine. New York: William Sloane Associates, Inc., 1947. Pp. 234. $3.50.

Since the middle ages, human thought in Western civilization has passed through eight basic revolutions, which may be characterized as follows:

1. From the conception of a universe around a flat earth, to the conception of a cosmos in which our spherical earth revolves around its sun in one of innumerable galaxies;

2. From government by dictatorial hierarchies, to democratic government, aspiring to be founded on the will of the people;

3. From magical alchemy and rule-of-thumb craftsmanship, to experimental research in physics and chemistry, with scientific engineering, and the resulting industrial revolution;

4. From superstition in medicine, farming, diet, and other biological fields, to scientific research via microscope, chemistry, and statistics, with resulting revolutionary improvements in public health;

5. From the dogma of sudden creation, to the theory of evolution;

6. From the dogma of the indivisible atom, to modern electronics and the resulting development of our electrical civilization;

7. From dogmatic pedagogy and propaganda, to mental tests and attitude scales, with resulting purposeful progress in education; and

8. From the Newtonian system of mechanics, to Einsteinian relativity, with its resulting release of atomic energy and the precipitation of the Atomic Age.

Dr. J. B. Rhine's new book, *The Reach of the Mind,* is a brilliantly clear, illuminating, highly readable, and yet profoundly scientific summary of a ninth basic revolution, which deserves to be appraised in the perspective of these other eight great modern changes in human thinking. This revolution consists in the scientific demonstration that man is fundamentally mind-centered rather than brain-centered. Dr. Rhine says:

> In the reconstruction of the findings of the sciences

> into a single universal scheme, the mind as a distinctive order of reality was left out. It had no place in the new mechanistic picture of the world. Wherever science came in, the tradition of belief in man's spiritual nature went out. Psychology became increasingly saturated with physical concepts. . . . There is no tolerance left in the sciences for anything like the nonphysical, or exclusively psychical reality which men once labeled the soul (page 10).

In demolishing this position (which Rhine shows on page 7 to have been dogmatic rather than scientific), Dr. Rhine summarizes the evidence which compels the open-minded seeker to take the following eight steps:

1. Mind-to-mind interaction (telepathy) occurs without a known physical medium.
2. Mind can enter into a cognitive relation with matter (clairvoyance) without the use of any known sensory-mechanical means.
3. This mental capacity is able to transcend space.
4. It also transcends the time dimension.
5. The extraphysical system of the mind exerts a significant influence on moving cubes (psychokinesis).[1]
6. Psychokinesis, like the other parapsychological faculties, is nonphysical in character (page 119).
7. Telepathy, clairvoyance, and psychokinesis are so closely related and so unified logically and experimentally that these forms of mind-matter interaction can be regarded as one single, fundamental two-way process (page 130).
8. These faculties are necessarily functions of the whole mind; the integral human mind must participate in PK and ESP just as it does in any other normal mental performance (page 152).

If Dr. Rhine has presented convincing evidence that these eight steps have been demonstrated (and the present reviewer believes that he has done so), the ninth great revolution in human thought has actually taken place already in research centers at various points in Western civilization and is now in process of diffusing to the leaders of thought who will incorporate it into our future civilization. If this is true, how does this parapsychological revolution compare in significance with the others listed above?

[1] The foregoing five points are summarized on page 108.

The other eight revolutions have been significant primarily in three ways. First, they have opened up for the human spirit vaster and vaster perspectives of reality in time, in space, and in telescopic and microscopic structure. Second, they have provided scientific insights which have given meaning to life, helping mankind to understand its relation to the universe. Third, these discoveries have revealed, and made usable, immense forces which mankind has been able to utilize in carrying out its purposes. The revolution summarized in Rhine's book consists in the scientific rediscovery of the human soul and of the spiritual universe in which it has its being. As compared with mechanistic materialism—or even with the physicalistic watering-down of that materialism (see page 10)—this demonstration of spiritual reality increases man's perspective to a literally infinite degree. As to the significance of existence, this revolution takes a civilization which seems in danger of breaking down into apathetic despair or cynical meaninglessness and reawakens the human mind to the immense adventures which lie ahead in the exploration of our spiritual environment. Finally, if steam pressure, electricity, and chemistry have placed vast powers at man's command, what of a revolution which demonstrates by laboratory methods that mind can contact mind without sensory communication, that mind can perceive matter without the aid of its senses and regardless of time and space, and that mind can control matter without physical contacts? As Dr Rhine keeps pointing out, these discovered powers are still rudimentary, just as early discoveries in the field of electricity were rudimentary. But now that they have been demonstrated, the methods of expanding our control over them begin to become clear. The revolution is under way. Who is to say that any of the other eight revolutions in human thought have approached in their significance the revolution precipitated by scientific parapsychology?

A word is in order at this point relative to Dr. Rhine's relation to this revolution. In this book, as elsewhere in his writings, he calls attention to the immense amount of research which was done in this field before he began his work at Duke, and to the investigations which have been and are being carried on in many universities, colleges, and research centers in various parts of the Western world.

But a dispassionate appraisal of the history of psychical research shows that more work has been done in the Parapsychology Laboratory at Duke than all the systematic research, in all other centers combined, up to the date when Dr. Rhine began his work. He has been the university-laboratory pioneer, who has developed and organized data which have compelled the acceptance of parapsychology into the world of organized science. To ascribe *any* revolution to any single individual or group would, of course, be an over-simplification. But Dr. Rhine may well be regarded as the Galileo, the Jefferson, or the Darwin of the Parapsychological Revolution.

Having presented the essentials of this revolution, Dr. Rhine goes on to develop his views on certain broader implications—which are highly stimulating and also wide open to controversial discussion. Dr. Rhine seems to this reviewer to be somewhat too prone to view the significance of the Parapsychological Revolution entirely in the light of psychology and mathematics. It would seem to this reviewer much safer to bring to bear all the light which can be found from ethnology, sociology, the comparative study of religions, and other pertinent disciplines. For example, when Dr. Rhine develops, in his Chapter 12, the ethical consequences of this revolution, he seems to expect that a recognition of man's spiritual nature will eliminate social conflict. Recent events in India do not seem to support this conclusion. Dr. Rhine also feels that if telepathy and clairvoyance could be made one hundred per cent certain, the result would be such effective secret service as to eliminate all war. But suppose that telepathic hypnosis were to be highly developed, and to be brought to bear upon international relations, would the result not merely be that warfare would be moved up onto the spiritual level, as well as being carried on chemically and biologically?

Dr. Rhine points out, very properly, that these parapsychological discoveries have crucial bearings upon the question of survival. But is not "survival" a space-time concept? If the human spirit transcends space and time, must we not substitute some much more fundamental concept instead of the superficial one of a continuation of space-time experience beyond bodily death?

Such questions merely suggest the highly stimulating nature of the problems raised in Dr. Rhine's book. They are subordinate in

significance. The basic fact is that this book summarizes, more clearly, adequately, and comprehensibly than any previous publication, what may well prove to be the most fundamental revolution which has ever occurred in the thinking of human beings.

HORNELL HART

CHALLENGE OF THE UNKNOWN by Louis K. Anspacher (with an introduction by Waldemar J. Kaempffert). New York: A. A. Wyn, 1947. Pp. 319. $3.75.

Any book should be appraised in relation to the audience for which it is intended. Books on parapsychology might be written for any one of three broad audiences: (1) persons potentially interested in the subject, but almost entirely unfamiliar with the literature; (2) amateur addicts, who are more or less familiar with psychical research; or (3) professional, or at least serious and systematic, investigators. The present book merits the attention of the first of these three audiences, but it may seem to be rather trite to members of the other two.

Dr. Anspacher writes with considerable charm, in the style of an essayist, often presenting stimulating ideas, but not achieving any very deep or new synthesis. The present reviewer found his interest somewhat aroused by part four of the book, entitled "What the Great Philosophers Have Said on the Question of Psychics." This section, which occupies about a third of the book, is a general review of the history of philosophy from the standpoint of its relation to a future life and the spiritual world.

Coming into the field as a layman, and apparently not having read with any thoroughness the more rigorous and critical discussions of psychical research, Dr. Anspacher shows considerable credulity about reports of psychic phenomena, and often allows himself to be swept along into spiritualistic enthusiasm on the basis of dubious evidence. For example, his discussion of the New Testament as a psychic document fails to take into account the more careful studies which have been published on this subject.

HORNELL HART

LETTERS AND COMMENTS

A PHYSICIST TELLS WHY HE ENTERS PARAPSYCHOLOGY

I believe that the following personal letter from a young physical scientist will be of interest to our readers. At my request the writer has agreed to allow its publication.—J. B. R.

Dear Dr. Rhine:

You have probably wondered why I have refrained from discussing my plans for the future. My silence has not resulted from any indecision regarding my ultimate purposes. I have hesitated to speak because I do not yet clearly see just how I shall attain those purposes. Since it appears that we shall be cooperating closely in one way or another in the coming years, I shall try now to give you the picture of my hopes with regard to parapsychology.

It was in the years 1943 and 1944 that I began to see clearly the answer to an old question. I have long believed that I was engaged in preparation for some specific but still unperceived life work. In 1943 two things happened in this connection: there was an exodus of nuclear physicists from the ordinary fields of war research, and the JOURNAL OF PARAPSYCHOLOGY announced the experimental proof of psychokinesis. While these events did not mark the beginning of my interest in atomic energy or in extrasensory perception, there can be no doubt that they focused my attention thereupon.

By late 1944 I believed that only these two subjects were of sufficient importance to hold my lifelong interest. When the bomb was dropped in 1945, the choice between them was made for me. On the one hand, my rather indifferent intellectual power would be lost in the flood of effort thereby to be loosed upon nuclear physics. On the other, to hopes for greater happiness among men, there had now been added fear as a spur to my interest in parapsychology. Another war seemed unthinkable and at the same time unavoidable unless man should find some new knowledge about himself. Thus it was that I turned to parapsychology. . . .

So much for the background. What are my aims? Broadly speaking I want to advance the state of man's knowledge in parapsychology as rapidly as possible. Here is a revolutionary science. My hope is to see the application of its benefits within my own lifetime.

To you who have labored patiently for more than fifteen years this must seem a grandiose objective. I wish to convince you that instead it is a reasonable program for action.

In the war which has just ended I learned a lesson which I vowed never to forget. It is this: that miracles can be performed by intelligence combined with unlimited funds. The miracle of this country's industrial production was plain for all to see. The still greater miracle of a five- to tenfold acceleration of scientific progress is something I was privileged to watch in intimate detail at the level at which that miracle was accomplished.

Can a similar miracle be brought to pass in parapsychology? There is, to be sure, the grave question of whether research on so evanescent a thing as ESP will yield to the methods which have proved successful in physical science. This question must be answered experimentally. But it is certainly true that ESP research can be hastened immensely by increasing the number of thinkers and by furnishing those thinkers with all of the mechanical and technical assistance which they can economically use.

One may grant the desirability of widespread and generous support for parapsychology and still doubt that it can be achieved in our time. But if we so doubt, then I believe we have not analyzed the situation clearly.

It was proper and necessary that the foundations of parapsychology should have been laid as they were—carefully, quietly, and so solidly as to be forever beyond destruction. Now we must recognize that the foundations are completed. We must shake off those habits of conservative thought which have been a necessary protection against a hostile world. It is time in history that we do so. You have recognized this fact yourself: *The Reach of the Mind* shows that. But perhaps you have not gone far enough in boldly demanding support for parapsychology. Why do I say this?

For several thousand years the religious spirit has ebbed and

flowed in man. The beach of history is littered with the vestigial debris of spring tides from the past. Just now we are witnessing a resurgence of that spirit under a unique threefold conjunction. We shall do well to study the rising tide and to make our plans accordingly.

What is this threefold cause which is flooding out materialism? It stems from our advance in physical science, and in this it may be considered a single cause. But it is resolvable in its operation as three causes which may be separately examined.

The first of these is the epistemological state of science itself. The complacent determinism of the last century has been utterly destroyed. However much the professional scientist may deprecate the popular mysticism of an Eddington, in all honesty he must recognize the uncertain nature of observation and knowledge as epitomized in the universally accepted wave-mechanical theory of the atom. Physical science is now philosophically prepared to accept psychic phenomena. Moreover, by its proclaimed principles, physical science must welcome the experimental proof of the existence of such phenomena.

The second cause for a revival of spiritual interest is the approaching culmination of the industrial revolution. Throughout history man has struggled to feed and clothe himself. In the past the acquisition of material necessities has seemed a worthwhile goal in itself. Now man has so successfully conquered his environment that he is rapidly approaching a period of abundance and ease. Instinctively, half-consciously, he is peering ahead and asking: "What next?" Instinctively, half-consciously, man *en masse* has begun to believe that mere living is not enough. In the past man has sought religion as a refuge from misery. Now he looks to religion as a refuge from boredom.

The third cause of the present trend to religion is the increasing interdependence of mankind. Our civilization can provide the good life for all, but only if there is harmony and understanding among men. We seek to escape from industrial strife and from atomic warfare. We yearn for unity under some authority which all men will gladly accept.

These three causes have set men to searching desperately anew for a reason for being. One has only to watch the feverish resurrection and re-examination of old religions to know how urgent are the spiritual needs of our time. One cannot but believe that, once known and understood, the present facts of parapsychology will bring the acceptance of that science on a scale and at a pace beyond our most sanguine wishes. (Indeed, herein lie frightening possibilities which we would do well to anticipate.) The world is more than ready for parapsychology: the world cries out for it.

What does all this mean in terms of practical support for research? It is this, I think: Are there billions for the will-o'-the-wisp of military defense? Then surely there are millions for this new science which is the best hope of man. If we are not properly supported, the fault is with us and not with the cause we advocate.

What does this mean in terms of my own activities? I shall base those activities on the purpose I have already laid down: to advance the state of man's knowledge in parapsychology as rapidly as possible. Because I believe that progress depends so largely upon adequate financial support, and because I believe that such support will be gladly given once the request is understood, I am forced to decide that my main efforts shall be devoted to telling others about the nature and possibilities of parapsychology. It follows that actual research can occupy only a lesser part of my time until the more urgent goal of a widespread acceptance of parapsychology shall have been achieved.

I have reached this decision with reluctance. I recognize that I am not well suited to the role of salesman. Moreover, I see a virgin field of endeavor in the utilization of physical science for the investigation of parapsychology. As a scientist, I am unhappy to be forced to forego the privilege of breaking ground in that field. Nevertheless, these considerations are outweighed by the need for a full-scale attack upon the problem as a whole. . . .

This letter is already long and I have barely mentioned my intention to conduct research in parapsychology. I must be content for the present to say that I have been formulating plans for such research. I hope to discuss these with you as the opportunity arises.

Sincerely yours,

ANONYMOUS

A METHOD OF CORRECTING FOR DICE BIAS IN EVALUATING PK TEST DATA

(A letter to the Editor of the *Journal of the Society for Psychical Research* by Prof. Robert H. Thouless, Cambridge University, England, reprinted with permission of the Editor and the author.)

Sir:

Discussions of experiments on psychokinesis seem sometimes to assume that psychokinesis can only be demonstrated if the dice used are unbiased. This is not the case. If each face is taken as target an equal number of times it can easily be shown by simple algebra that the deviation from mean chance expectation of the total number of hits is independent of the bias of the dice.

Let a, b, c, d, e, f be the probabilities of the faces 1, 2, 3, 4, 5, and 6 respectively turning up in any one throw. Then $a+b+c+d+e+f = 1$. Suppose that in $6N$ throws during which each face has been aimed at a target N times, one obtains X hits. If the dice were assumed to be unbiased, the mean chance expectation would be N and the deviation from expectation would be $X - N$. These values will be exactly the same if the dice are biased, for the mean chance expectation of hits on the one-face will be Na, on the two-face will be Nb, and so on. The total number of hits expected will be, therefore, $N \times (a+b+c+d+e+f)$ which equals $N \times 1$. The expected number of hits and, therefore, the deviation of any given score from expectation will thus be independent of the bias of the dice if each face is taken as target an equal number of times.

The variance will not, however, be independent of the bias of the dice. If the dice were unbiased, this would be taken to be $6N \times 1/6 \times 5/6 = 5N/6$ (if $6N$ is the number of throws). But if the dice are biased, the variance will be different for different target faces; for example, with the one-face as target it will be $N \times a \times (1-a) = N(a-a^2)$. The variance of the total will be the sum of the variances on different target faces; that is, it will be $\Sigma N(a-a^2) = N(\Sigma a - \Sigma a^2)$. $\Sigma a = 1$, so the total variance is $N -$

$N\Sigma a^2$. If the dice are unbiased, this quantity Σa^2 becomes $1/6$ and the variance will be $5N/6$ as above.

This correction for bias in the variance is not generally made. This is unimportant for two reasons. The effect of bias is to reduce the variance, so ignoring the correction means that the variance tends to be overestimated and therefore the significance of any deviation is underestimated, which is an error on the right side. Also the correction required is very small. For example, the relative probabilities of falls of the different faces in the results shown in Table 2 of "A Large Series of PK Tests" by Gibson, Gibson, and Rhine in THE JOURNAL OF PARAPSYCHOLOGY, December, 1943, are: .160, .163, .165, .166, .171, and .175. The sum of the squares of these is .1668, which makes the true variance $.8332N$ instead of $.8333N$ as calculated by the ordinary formula. The error of rather more than .01% is, of course, altogether negligible.

That results obtained when each face is target an equal number of times are independent of the bias of the dice is perhaps sufficiently obvious. Some of the North Carolina results have, however, been obtained under conditions in which this equality was not secured. They are commonly treated as if this inequality rendered the results necessarily dubious. It is, however, possible to treat them also by a similar method which renders the deviation independent of bias, although the method I suggest has not been used by those who have published such results.

Let A, B, C, D, E, F be the number of times that the targets 1, 2, etc., have been thrown for. If $A, B,$ etc., are unequal and the dice are biased, it is obvious that the probable total score ($Aa + Bb$ + etc.) will not be independent of $a, b, c,$ etc. But if the scores for each target face are weighted by factors inversely proportional to the number of times that target face has been aimed at, one obtains a new total which will be independent of bias. The weights, for example, could be $M/A, M/B,$ etc. The chance expected score for the one-face will then be $M/A \times Aa = Ma,$ and the chance expected total score will be $M\ (a+b+c+$ etc.$) = M$. That is, it will be wholly independent of the bias of the dice.

But by thus weighting the scores we have also weighted their

variances and the change of variance must be taken into account in estimating significance. By weighting the score on target 1 by multiplying by the factor M/A we have weighted its variance by the factor $(M/A)^2$. Since the original variance of throws on target 1 was $A(a-a^2)$, the contribution of its variance to the weighted total score is $(M/A)^2 \times A(a-a^2) = M^2(a-a^2)/A$. The variance of the weighted total will, therefore, be $M^2\Sigma[(a-a^2)/A]$.

To illustrate this method 1 will take the data given in Table 2 of the article by Gibson, Gibson and Rhine referred to above. This was a series of witnessed experiments by three subjects with a total of 24,132 throws of six dice each, showing, when worked out in the ordinary way (ignoring bias), a total deviation of +1057 with a critical ratio of 7.46. Examination of the table shows that subjects most commonly chose to throw for high faces and that these were the ones that fell uppermost most often. The question to be decided is whether, on the hypothesis that the high faces fell uppermost most often as a result of bias, the observed deviation was due to the subjects choosing as targets those faces which were favored by bias. The exact numbers for scores on each target face are not given in the article, which gives only average scores, but Professor Rhine has very kindly sent me the details of exact scores.

The results are as follows:

Target faces	1	2	3	4	5	6	Total
No. of single dice-throws ..	24804	12816	15444	9684	39168	42876	144,792
Hits	4098	2150	2630	1656	6923	7732	25,189

One would have preferred to estimate the bias of the dice from all the falls (whether hits or not) but since these were not recorded we must estimate it from the relative values of the percentage of hits on each target face. It is no objection to this procedure that the relative frequency of hits might be determined by preferential action of PK on some target faces. We are testing the null hypothesis that there is no PK and on this null hypothesis relative frequencies of fall will be entirely determined by bias. The question at issue is whether these results can be explained on this null hypothesis, so for testing significance the null hypothesis is assumed.

The percentages of hits on each target face are:

16.54 16.78 17.04 17.10 17.67 18.04

Dividing the values by their total (103.17) gives us the following estimates of bias:

Target face	1	2	3	4	5	6	Total
Estimated bias	.1604	.1627	.1650	.1657	.1714	.1748	1.0000

These are the estimated probabilities of each face on a single throw. It will be seen that (as is usually the case) the general direction of the bias is in favor of the higher numbers.

In order to weight the score to reduce it to the value it would have had if each target face had been thrown for an equal number of times, we must multiply the observed deviations by a weighting factor inversely proportional to the number of throws on that target. The denominator chosen for the weighting factors makes no difference to the final result, but it will be convenient to make it the average number of single throws per target; that is, 24,132. The weighting factors will, therefore, be 24,132/24,804; 24,132/12,816; etc.

To three decimal places, these weighting factors are:

.973 1.884 1.562 2.495 .616 .563

Multiplying the observed number of hits on each target face by these factors we get:

Target faces	1	2	3	4	5	6	Total
Weighted hits	3987	4048	4109.5	4127	4265.5	4352	24,889

This weighted total of 24,889 is smaller than the observed total of hits, 25,189, since the preponderant influence of the favorite throws of 5 and 6 on which scoring was heaviest has been eliminated. This may be regarded as the score that would have been obtained if all targets had been aimed at the same number of times and the total number of throws was still 144,792. It may be directly compared with the mean chance expectation of one-sixth of 144,792 (24,132). The deviation from expectation is +757 (instead of 1,057 as calculated from the unweighted total).

An exact estimate of significance can be made, as suggested

earlier, by allowing for the effects on the variance of the bias and of the different weighting of the hits on different faces.

For this we need to calculate the quantities $(x-x^2)$ where x is the bias for each face. We have:

x (bias)	.1604	.1627	.1650	.1657	.1714	.1748
x^2	.0257	.0265	.0272	.0275	.0294	.0306
$(x-x^2)$	.1347	.1362	.1378	.1382	.1420	.1442

This must be multiplied by the weighting factor for each target face; that is, by M/A, M/B, etc., where M is the mean frequency of target faces. This gives:

$(x-x^2)$ × weighting factor	.1311	.2566	.2152	.3448	.0875	.0812

Total = 1.1164

This total is the quantity $M\Sigma[(a-a^2)/A]$ which multiplied by M gives the variance.

$$1.1164 \times 24{,}132 = 26{,}941$$

The standard deviation will be the square root of this, which is 164. This gives a critical ratio of $757/164 = 4.62$. This corresponds to a chance probability of less than .00001 which, though less impressive than that calculated when bias is ignored, is still more than adequately significant.

The object of this letter is to point out that PK results are not necessarily vitiated by inequality of target faces and to show how they can be treated in a way that makes the results independent of bias in the dice. It remains true, of course, that if the subjects are given free choice of target face, such results might be explained by precognition. For this reason and also because statistical treatment is much simpler, it is to be hoped that future investigators will adopt the method of equalizing target faces.

Yours faithfully,

R. H. THOULESS.

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

*For a simple description of those terms marked by an asterisk, as they apply to the ESP test data, see Chapter VIII and the Appendix of *A Handbook for Testing Extra-Sensory Perception* by C. E. Stuart and J. G. Pratt. Further explanation may be found in any elementary statistical text.

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

CALL v.: To attempt to identify by ESP a stimulus object or a mental state of an agent.

CALL n.: The response described above; also the resulting selection.

*CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.
CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.
CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI-SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

*CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)
CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards, five of each suit.

*DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibility of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

*P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

*SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

*SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. Common criteria are: a probability value of .01 or less, or a deviation in the expected direction such that the critical ratio is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.